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ABSTRACT

This manual is a technical resource for helping drug treatment program directors decide whether to do a follow-up of a client population and, if so, how to do one. Each chapter was written by a particular set of authors. However, the points contained in the chapters had to receive the general agreement of a committee whose membership included representatives not only from the research community, but also from the National Institute on Drug Abuse (NIDA) and the treatment community. After completing the first three chapters, the reader should have the information needed to determine whether to do a follow-up, and how to plan it in detail and make staffing assignments, if the decision is to proceed. The remaining chapters provide detailed guidance for implementing each of the research stages. A few of the areas covered are: techniques for successfully locating respondents, interviewing techniques, confidentiality, guidelines for preparing the data for analysis and conducting the analysis, and suggestions on how a report might be organized. The appendix includes many specific examples of interview questions and interviewing training materials. (Author)

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CONDUCTING FOLLOWUP RESEARCH ON DRUG TREATMENT PROGRAMS

CG 012 346

U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE · Public Health Service · Alcohol, Drug Abuse, and Mental Health Administration

CONDUCTING FOLLOW/UP RESEARCH ON DRUG TREATMENT PROGRAMS

1406

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1977

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FOREWORD

The history of the drug abuse field shows that real innovation and creative contributions to our fundamental knowledge about drug abuse are more likely to come from creative individuals working with specific problems in local programs than they are from massive national studies. My own experience with the local Washington, D.C., heroin problem taught me that it is easier to grasp reality and put it on paper when considering a local heroin epidemic than it is at the Federal level.

No issues are as important to local program staff as the questions of what happens to clients in treatment and what happens to them when they leave treatment programs. It is often difficult to turn intense interest in these questions into practical studies because the local staff lack the technology to carry out the study. To meet this need, we, at the National Institute on Drug Abuse (NIDA), have developed two handbooks which bring the most sophisticated evaluation and followup methodologies within the grasp of the serious treatment program staff.

The first volume, written by Guess and Tuchfeld, is called the Manual for Drug Abuse Treatment Program Self-evaluation. It is fully compatible with the Federal CODAP system, but it can also be used with virtually any routine data system.

The second volume extends this work to the even more complex area of followup studies. The authros and editors are among the most distinguished research scientists in the drug abuse field. They have each conducted and published historic studies which are now changing the way we think about drug abusers. In this new volume, they have taken complex theories and controversial techniques and distilled the practical essence for use by local program staff. The more sophisticated researcher can use this volume to assess the basic literature in the field. The more practical drug abuse worker can feel secure in the knowledge that this deceptively simple guidebook has been put together for him by the best minds in the field.

Because our drug abuse field has been on the forefront of knowledge development in the program evaluation and followup fields, both of these volumes will also prove useful to practical researchers in other fields in which intervention programs are designed to produce measurable behavioral changes.

When you do a study in your program, I hope you will let us at NIDA know what you found useful in this book and in what areas it could be improved. Also, please let us know what your study results are. We want to know what you learn about your program.

Robert L. DuPont, M.D.
Director
National Institute on Drug Abuse

PREFACE

There are nearly 2000 Federally-funded programs in the United States today, providing a variety of treatments to drug abusers. Since most of the directors and staff of these programs have an interest in what happens to their client populations after treatment--and in demonstrating to funding agencies, among others, that what happens represents an improvement as a result of treatment--there has been a continuing interest in conducting follow-ups of former clients. However, given that most program personnel have not been trained in research, a lot of effort may be wasted if follow-up studies are not undertaken properly.

Recognizing this state of affairs, Dr. Robert DuPont as Director of the National Institute on Drug Abuse (NIDA) commissioned a committee--comprised largely of experienced researchers in the drug abuse field--to develop a manual to guide program personnel in the conduct of follow-up studies. This document is the product of that committee. It is intended as a technical resource to be used in deciding whether to do a follow-up of a client population and, if so, how to do one. Its recommendations reflect the combined judgement of the committee members.

The Approach of the Committee

A word about the manner in which this volume was written may help the reader to understand the judgements reflected here. Each chapter was written by a particular set of authors, as the credits indicate. However, the points contained in the chapters had to receive the general agreement of the larger committee, whose membership included not only representatives from the research community, but from NIDA and the treatment community, as well.

The members first convened in February, 1976 to agree upon a list of topics for inclusion in the volume, generate a number of specific ideas, and assign authorships. They reconvened a few months later to review first draft chapters and consider revisions. It was at this stage, in particular, that a number of conflicting viewpoints were aired and generally resolved. This integrative process was important to the development of what is hoped to be a comprehensive and balanced approach to the problem. The necessity of developing a consensual set of recommendations was, in fact, the major justification for a committee doing what it is said a committee should never do--write a book. In the judgment of the editors it was a worthwhile, if sometimes frustrating, process: and the final product benefited from the multiple viewpoints represented on the committee.

Upon completion of the substantially revised second-draft chapters, the editors took a heavy hand in the interest of brevity and consistency in style and level of detail. The authors then received one more chance to preserve cherished ideas and phraseology.

Who Should Read This Manual?

It has been assumed that the readership of this manual is comprised of professionals who are not trained researchers, although researchers may have an interest in its contents. The manual is directed toward two types of readers: (1) individuals who must decide whether to initiate a follow-up study, presumably the directors of treatment programs; and (2) the individuals who will carry out the research.

While the primary intent of this manual is to guide the directors and personnel of drug treatment programs, much of what is contained here could be generalized to other treatment clientele, such as those from alcohol and mental health facilities. Therefore, this manual can be used by staffs of other service agencies as well. The portions of the manual which are inappropriate for such readers--such as the suggested interview content--should be obvious to them as they proceed.

How to Read the Manual

For purposes of deciding whether or not to undertake a follow-up study, the reading of Chapters 1 through 3 will suffice. Chapter 1 provides a general introduction and outline of the goals which can and cannot be accomplished with follow-up studies. These issues are particularly important, since the establishment of realistic goals and expectations seems to be one of the hardest steps when undertaking such a venture. Chapter 2 reviews the issues which should be taken into account in planning for such a study, including the estimation of personnel and financial resources. It also provides an overview of the various stages of the research process, stages which are then discussed in much more detail in the subsequent chapters.

Chapter 3 discusses the major types of alternative research designs, and what kinds of questions can be answered with each. Selecting a design based upon your interests and available resources is an important first step. Also covered in this chapter are the techniques and considerations to be taken into account in drawing a sample of clients for follow-up.

After completing the first three chapters, the reader should have the information needed to determine whether to do a follow-up, and how to plan it in detail and make staffing assignments, if the decision is to proceed.

The remaining chapters provide detailed guidance for implementing each of the research stages. Those responsible for actually implementing such a study are advised to read the entire manual through quickly, and then to study each chapter more carefully at the appropriate stage. It may also prove beneficial to have staff members who will be working on the project read the chapters relevant to their tasks.

Chapter 4 describes the process of developing the content for your interview or questionnaire, and general principles and examples are provided. Since the selection and/or development of good questions is a particularly difficult and time consuming task, specific questions are included in an appendix to illustrate many of the questions most often included in follow-up studies. They are presented for consideration with the recognition that you may wish to include questions about topics which are not included here and/or use different measures for some of the topics which are included. Having some specific suggestions, however, will at least provide a starting place for everyone and will probably meet most of the measurement needs of most readers.

Once a research design has been chosen, a sample of clients drawn, and an interview developed, the next major stage is the actual collection of data. Chapter 6 suggests techniques for successfully locating respondents and outlines some of the difficulties which should be anticipated in the process. Chapter 7 deals with interviewing and includes recommendations for the selection, training, and supervision of your interviewers. Chapter 5 points out the necessity for protecting respondent confidentiality during the locating and interviewing stages, and gives some very practical suggestions for providing such protection.

Some alternatives to gathering data directly from clients are discussed in Chapter 8--primarily the use of institutional records. Not all programs will want to seek data from such sources, but this chapter gives an overview of the types of institutional records which are generally available and of the necessary procedures for accessing each type.

Once the full set of data has been secured, there remain the tasks of analysis and writing. Chapters 9 and 10 provide guidelines on how to prepare the data for analysis and how to actually conduct the analyses. These, of course, are complex topics about which entire courses are taught. Nevertheless, the authors provide some fairly simple, general guidelines for individuals who are not experienced at data analysis.

The concluding chapter suggests how a final report might be organized and provides some suggestions and caveats about interpretation. It also reviews some of the ways in which the results might be used by the treatment program.

Acknowledgements

The editors are indeed grateful to all of the committee members for their time spent in the development and writing of this volume, and for their cooperativeness and flexibility at the editorial stage. Trying to integrate the ideas and writing of so many people can be difficult at times--particularly for the authors--but everyone approached the effort with good humor as well as dedication.

It is impossible to thank all of the many programs and agencies who field tested this manual, but we wish particularly to mention Rev. Richard Hamilton and Dr. Eugene Farrell at the Maryland Drug Abuse Administration, and Mr. John M. Lord and Dr. Claudio Toro at the Regional Drug Abuse Program, Department of Psychiatry, University of Alabama in Birmingham. Special thanks should also be given to Mr. Michael Gold at the Achievement Through Counseling and Treatment Program, Philadelphia, Pennsylvania, and to Mr. James E. Rivers at the Comprehensive Drug Program in Dade County, Florida.

We are additionally grateful to the Addiction Research Center at Washington University in St. Louis for providing a home for the work of this committee. Dr. Samuel Guze, Chief of the Department of Psychiatry, and Dr. Donald Goodwin, Director of the Center, provided valuable cooperation from the outset.

We hope that this document will prove of assistance to the directors and staff of many treatment programs across the country, and ultimately contribute to the improvement of the human services they provide. We hope that it proves useful to you, specifically, and we wish you good luck in your follow-up venture.

L. J.
D. N.
L. R.

Chapter 1

INTRODUCTION TO THE USE OF FOLLOW-UP STUDIES

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What to Expect

It should be stated clearly from the outset that follow-up studies are among the most complicated of all research studies to conduct and to interpret correctly. New research techniques are still being developed and the many experienced research scientists continue to debate the merits of the techniques which currently are available. Therefore, it is not possible for this manual or any other, for that matter, to transform its readers into masters of a foolproof method. The available methods are themselves far from foolproof and even to provide a complete understanding of the existing ones--particularly of the analytic techniques -- is well beyond the scope of any single volume.

What, then, is a realistic expectation for the reader? It is for you to become familiar with the possibilities and limitations of follow-up studies, with the basic principles which should be followed in the design and interpretation of such studies, and with some specific techniques of proven effectiveness. You should not expect to learn the more sophisticated analysis techniques. only the simpler variety can be covered here.

WHAT IS A FOLLOW-UP STUDY?

Follow-up studies include any research in which a sample is identified at one point in time, and then later located personally or in records to learn what has happened in the interval. In this volume, it will be assumed that the occasion for identifying the individual initially is his enrollment in or his departure from a drug treatment program. The length of enrollment which makes him eligible for inclusion in the study will be determined by those directing the study. Similarly, the interval of time between a person's departure from a program and his eligibility for inclusion in the follow-up is a research decision, although six months would likely be the minimum interval you would want to use.

The length of time to be spent in relocating and reevaluating subjects is another important decision to be made in the design of follow-up studies. Most such studies are confined to some discrete time period such as three months or six months. While in principle it is possible to conduct a continuous study of the ongoing stream of clients who are leaving the program, it seems unlikely that many will find this an attractive approach. It is expensive; sometimes the results never get analyzed when there is no fixed end-point; and since the subjects encounter different labor markets and different opportunities to use drugs due to historical changes, their outcomes may not be comparable.

Another decision about time that must be made in a follow-up study is the number of past years from which departing clients are to be sampled and studied. At the one extreme, you could sample from all clients who have completed the program since its inception. At the other extreme, you might study only clients who entered treatment during a specific six months. If you think that your clients have changed over time, you may want to compare clients seen at the beginning with those seen recently. The decision will depend in part on the questions you want to answer and in part on how big your program is. Small programs will have to accept subjects seen over a longer period to get enough cases. When it is possible, however, the study is easier to analyze when clients all entered about the same time and are all followed over roughly the same period of time so that the same length of follow-up interval and the same historical period will apply to all of them.

OBJECTIVES OF FOLLOW-UP STUDIES

It is very important for anyone thinking about conducting a follow-up study to be aware early of what s/he expects to accomplish. There are several basic objectives to which follow-up studies can be addressed, and they differ substantially in the complexities and difficulties which will be encountered in accomplishing them.

Information on Events after Treatment

Perhaps the most attainable objective is to develop some systematic information on the fate of your clients after they leave your program. Leaving aside the question of whether your program has had any impact, or "enough" impact, you can learn some valuable information about the natural history of your client population over some defined period following their treatment experience. By using a representative sample of the client group, however you choose to define it, and by locating and interviewing them, you can get a good notion of how many found legitimate employment, how well they are functioning in their work, how many stayed out of trouble with the law, how many married and had children, and so on. You should also be able to get a reasonable notion of the proportions who continued drug use and who sought help from drug programs elsewhere after leaving your program.

Such information can be helpful in formulating program goals or revising program emphasis. For example, if it turns out that locating a legitimate job is a central problem for most of your clients in the year after treatment, presumably you would want to consider placing a greater emphasis on job counseling, job placement, and perhaps job training. Whether a greater effort in this direction will ultimately prove successful is another question, but at least the need has been clearly identified.

Another way in which such information may prove helpful is in providing some basis for forewarning your current and future clients of the problems they can expect to encounter after leaving the program. You would obviously want to use judgement in determining what information can be incorporated constructively by clients and what information might be discouraging; but certainly some of it can be used constructively.

Descriptive Information on Services Delivered

Another type of information which also may prove useful for program improvement concerns the quantities and types of service actually received by your clients during their enrollment in your treatment program -- such things as amount of individual counseling received, amount of group counseling received, and so on. Remember that the prescribed regimen of treatment may be quite different from the treatment services actually delivered. Therefore, it is useful to gather systematic information on the latter so that the reasons for discrepancies can be explored, and perhaps some improvements made.

Client Reactions To the Program

In a follow-up study you have a particularly good opportunity to get a view of the program from the client's perspective. While you could ask clients during treatment about their views on the strengths and weaknesses of the program, it is quite possible that better information can be derived at some point after treatment, when they are no longer dependent upon the evaluations of the program staff and when they have had a little time to gain some perspective on the whole experience. It seems reasonable to suppose that the clients themselves can come up with some useful information about existing problems and possible improvements. Questions can be asked about overall satisfaction or dissatisfaction with the program, about satisfaction with various specific aspects of the program, about the degree to which various problems were encountered during treatment, and so on. You can also learn more about the reputation of the program among users "on the street," which is likely to be an important factor influencing the number and types of people who seek treatment. Finally, you may learn more about clients' reasons for dropping out of the program early.

An Occasion for Client Accounting

It would be desirable for any service delivery program to take a periodic accounting of the characteristics of the clients it is serving and of those who withdraw early, and to note any changes in these characteristics over time. A follow-up study is not required to accomplish these tasks. However, the occasion of conducting a study may also provide the stimulus and mechanism for doing such an accounting, particularly if one has not been done for some time. Obviously changing trends in the composition of the client population or in the types of clients who chose to leave treatment early are likely to have implications for the program's content and perhaps the nature of its staffing.

Evaluating the Overall Effects of Treatment

The benefits just discussed can be derived from virtually all follow-up studies, and ironically they are some of the most often overlooked. When people think about follow-ups, it seems that their sights are immediately set on quantifying and evaluating the overall impact of the program.

Of course, there is nothing intrinsically wrong with having such a goal. The problem comes in failing to realize that attaining it may be unrealistic in many cases, because of a shortage of resources needed to do the research and/or because of the unavailability of the data necessary for developing unambiguous conclusions.

To put it at its simplest, to determine the effects of some intervention like treatment, you must determine what would have happened to the clientele in the absence of that intervention. Since they were in treatment, it is not possible to know what would have happened to the clients without treatment. The best approximation is to study people who are just like them in all other respects except for treatment. This is the notion of a "control group."

The way that scientists usually try to assure that their control group is like their treatment group in all relevant ways is to randomly assign subjects to the treatment and non-treatment conditions at the outset -- a procedure which exists in very few, if any, drug treatment programs. Without the random assignment procedure, it is extremely likely that individuals who are selected for treatment are systematically different from those not selected. Similarly, those who self-select themselves into an untreated control group by not showing up for treatment after admission or dropping out early are very likely different from clients who continue to attend. These systematic differences (perhaps in background characteristics, legal status, motivation, psychological stability, etc.) between those treated and those not, may account for any differences observed in their "success" rates. It is, of course, possible to try to determine in what ways the treatment group and the control group were different initially, and then to control for those differences in the analysis or to try to "correct" for them statistically. However, you never know for sure that you have identified all of the important initial differences between the treatment group and the control group, and just one undetected difference may be the one accounting for the entire difference in the success rates of the two groups. Therefore, drawing causal inferences from follow-up studies about the effects of drug treatment programs -- or for that matter, any other intervention in which random assignment is not used -- is always an uncertain business.

One important thing can be determined in this regard, however, and that is the upper limit of program effects. If you determine that only 50 per cent of all your clients secure legitimate employment within a year after leaving treatment, then you know for certain that at best your program accounted for half the clientele's attaining legitimate employment. The upper limit may be useful to have, but your inability to be more specific about the amount of program impact can be most frustrating if you start out with expectations of getting some definitive and precise answers.

Comparing the Effects of Different Programs

While you may not have an appropriate non-treatment control group against which you can compare your results, you may be able to compare your findings with those from similar programs. This is feasible only if you have comparable client groups and evaluate them after an interval of the same length, using the same criteria--which generally means that the two programs must agree in advance to use the same study design. However, if you wish to compare your program's results with the published results of another program, you should communicate with the study director at the other program to learn exactly how his study was done before beginning your own. What can be estimated from this type of design is not the absolute effect of either treatment program, but rather their relative effectiveness one to another.

Comparing Modalities in Your Own Program

Just as a treatment group can be compared to an untreated group or to a group treated in a different program, so can groups treated by different modalities in the same center be compared to one another. Again, what is being evaluated is the relative effectiveness of different treatment modalities, rather than their absolute impact on clients.

Of course, without random assignment of clients to treatment modalities (or to programs, if entire programs are being compared), the interpretation problems are formidable, since there are likely to be systematic differences in the types of people who enter different treatment regimens or different programs. Nevertheless, the urge to make such comparisons is usually too strong to be resisted; and, as long as the ambiguities in interpretation are kept firmly in mind, such comparisons may provide some useful perspectives.

SETTING REALISTIC GOALS

Perhaps the most important stage in the evolution of a follow-up study is at the very beginning, when its sponsors and directors form their goals and expectations for the study. Since the adoption of unrealistically high expectations of such research is almost a universal phenomenon, probably the most valuable piece of advice that can be given is to assume modest and realistic goals. Follow-up studies can do some valuable things for you; but there are many things that they cannot do — particularly when a relatively small number of clients is being studied in a single treatment program, with limited comparison groups available. A clear realization of that fact from the outset can prevent some disappointments, as well as some missed opportunities.

A careful reading of Chapter 3 on design alternatives should give you a good notion of which among the many goals is realistic for you within the constraints that you have. Among the constraints discussed there are the availability and use of control or comparison groups and the use of after-treatment data only (which implies a shorter time for the study) as distinct from data from before and after treatment.

If you feel that you can afford the monetary and staff costs needed to conduct a reasonably adequate follow-up study, at a minimum you should be able to get some valuable information on the natural history of your clients after treatment and on their experiences and feelings about your program. Whether, beyond that, you might be able to determine the relative efficacy of your different treatment modalities, or the relative or absolute efficacy of your program(s) is quite another question. In most cases these will be quite difficult, if not impossible, goals to attain. However, after you have read the next two chapters, you probably will be in a reasonable position to determine if you can. If you still remain uncertain about what you can realistically expect to accomplish with a follow-up study, however, that is an appropriate point at which to stop and seek the assistance of an expert in follow-up and evaluation research.

THE IMPORTANCE OF IMMEDIATE EFFECTS

This volume concentrates on the behavior and experiences of clients after leaving treatment. However, the more immediate effects of the program on client behavior and satisfaction during the treatment period certainly should not be overlooked. Such things as reducing drug use, improving health, decreasing criminal behavior, increasing employment and/or education, and generally improving the life experience of the client are all valid outcomes during treatment as well as after. In emphasizing outcomes after treatment in this volume, it is certainly not the intention of the authors to devalue the more immediate effects of treatment; and the reader should be cautioned not to do so either. *

In fact, it is quite possible to integrate a follow-up evaluation which has been planned in advance with an in-treatment evaluation. Many of the interview items suggested in this volume for use at admission and follow-up could also be used in interviews at intermittent intervals during the treatment period.

COMPARING COSTS AND BENEFITS

Once you have determined the goals which you can realistically expect to accomplish and the costs to the program of conducting a follow-up study adequate to accomplish them, you will want to take a careful look at the overall desirability of the venture. Follow-up studies will not make sense for all programs. For instance, you may already be accomplishing many of the same objectives less expensively through other means. Similarly, the program may be too new or may have changed too recently for an adequate sample to be drawn yet. On the other hand, external realities related to funding or political support for the program may dictate that such a study be done no matter what. Obviously, these are all factors which you will have to weigh in determining whether and when a follow-up study is desirable for your program. Since undertaking such a study is likely to be a sizeable task, you should consider these issues carefully before setting out.

THE USE OF OUTSIDE RESEARCH GROUPS

Two basic approaches are available for implementing such a research study -- directing and conducting it within your program or contracting with an outside research group to conduct it. The factors to be weighed in choosing between these arrangements are elaborated in the next chapter and, therefore, will not be reviewed here. However, should an outside contractor be your choice, you might want to consider whether there are cost advantages, and possibly other advantages, in joining with one or more other treatment programs in your area for the purchase of the research services. Not only could the fixed costs of developing interviews and procedures be shared by several programs, but you also would have some comparable data from other programs with which to compare your own results.

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* A volume recently published by the National Institute on Drug Abuse, Manual for Drug Treatment Program Self-Evaluation, provides a guide to conducting in-treatment evaluation studies. It can be ordered by writing to the Clearinghouse, National Institute on Drug Abuse.

With or without the participation of other programs, you would be well advised to establish a written contract with any outside research unit you might use. Obviously such a contract should be specific as to costs, time schedule, and expected products. You will probably also want to specify that completed interviews and data cards or computer tapes will be turned over to you at the end of the contract in case you want to reanalyze the results at a later date. If you hope to follow the same clients again later, you will also want copies of all the records used in locating them for interview and a key that will enable you to link the interview to the specific person interviewed. At the time of writing the contract, you should also make explicit what, if any, rights the contractor has to publish independently from your data or to identify the results as coming from your program. These are all issues which may prove important to you at a later date and which, therefore, should be clarified at the outset.

Chapter 2

PLANNING A FOLLOW-UP STUDY

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A Range of Decisions

This chapter provides a brief overview of the considerations fundamental to planning a follow-up study. It outlines the basic steps necessary in any such study, and discusses some of the secondary problems which the decision-maker will need to consider in formulating the research design and in mapping out the best use of available resources.

These considerations vary in complexity. They range from decisions about such matters as who should collect the follow-up data and from whom to collect it, to decisions about what kinds of data will be needed and what data collection methods should be used. Later chapters will deal with most of these questions in greater detail. The present concern is to provide a general introduction to them, and show how each decision can affect the others.

STEPS IN PLANNING A FOLLOW-UP STUDY

1. Defining the Research Problem

An agency may begin with a general question like "what happens to our clients after they are discharged from treatment? ", but such a question will be too general to guide the research. Research questions must be specific. They can be simple: for example, "do the clients have any suggestions as to how our program could be improved? " or "how many are using heroin again? ". Or they may be complex: for example, "do clients on methadone maintenance show less criminal behavior than those treated in a drug free program? "; or, using drug use, crime and employment as criteria, "are clients more improved after treatment than they would have been without treatment? " It will be seen in later chapters that it may be extremely difficult to obtain clear answers to these latter, more complex questions.

2. Defining the Audience

It is valuable to analyze who will actually use the answers to the research questions, and for what. At one extreme, the intention may be for the agency itself to use the answers. For example, the agency may wish to decide whether more effort should be made to help clients find employment. In such a case, the decision-maker can feel free to define what evidence he will consider sufficient to answer such a question for the intended purposes. The decision may be that not many clients need to be followed to provide an answer sufficiently definite for agency use. At the other extreme, if the audience for which the research answers are intended is the scientific community at large, or a funding agency, greater accuracy of estimation may be required and a much more rigorous research design may be needed, if credit is to be given to the findings.

3. Determining What to Measure

The research questions to be addressed in the study should be stated at a sufficient level of specificity so that the variables of relevance can be chosen. A concept such as "improvement" is not adequate, unless the dimensions on which improvement is to be measured are also defined.

Independent, Dependent, and Control Variables

Researchers generally speak of independent, dependent, and control variables. The dependent variables are outcomes which are to be predicted or explained in the research. The independent variables are those which the researcher thinks may predict or explain the dependent variables. By contrast, control variables are most frequently conditional; they are thought of as things which, if controlled, may make a difference in the relationships discovered between the independent and dependent variables. As an example, in a drug follow-up study, the dependent variable might be employment, and an independent variable might be the length of treatment. If you then asked whether the relationship between these two variables differed for clients of different ages, age would be a control variable. Controlling for a third variable, like age, may cause an observed relationship to intensify, to diminish, or to be more apparent in some subgroups (like older clients) than in others.

Baseline and Treatment Variables

You will also see references in the literature to baseline variables, treatment variables and post-treatment variables. Baseline variables are those which describe the client prior to or at the moment of entering treatment. These are often selected as control variables. Treatment variables describe the treatment he received, and are often the independent variables. Post-treatment variables, of course, are variables with a later time reference.

Outcome Variables

Post-treatment variables can be divided into two categories: first, outcome variables, those the project intends to use as descriptions of adjustment or criteria of success, such as employment, criminal behavior and drug use; and second, other variables to describe post-treatment conditions or experiences which may help to explain the outcome variables, such as the cost or difficulty of obtaining drugs.

It is important to define these variables very early in the plans for a research study. The baseline and treatment variables may already exist in client records, and these data can be abstracted and used as predictors of outcomes. If such data are not available in existing client records, they will have to be obtained, either at intake or in the follow-up. However, it is greatly preferable to have the baseline and treatment variables measured before the follow-up actually commences. If the baseline and treatment variables are not measured until the follow-up commences, it is possible that the outcome might influence the client's answers about what he was like before treatment or what the treatment experience itself was like.

4. Deciding on the Desired Degree of Accuracy

It is imperative to evaluate how costly errors would be. Suppose your research objective is only to find out whether there may be a need for more job counseling and job placement activities in your agency. Suppose further that a small sample is followed to obtain the answer, and it turns out that 50 per cent of the respondents report difficulty in getting a job. You would not be justified in concluding that exactly 50 per cent of all your clients had such difficulties, but you would know that a fairly large proportion are having them. You may not care whether the exact number is 40 per cent or 60 per cent. Therefore, for the purposes of your decision, you may consider this enough information on which to make a decision to reallocate resources to provide more help in job counseling.

At the other extreme, suppose an agency offers two kinds of treatment, and wishes to report to the scientific community which is better. Or the agency may even wish to consider terminating the less effective treatment and doubling the more effective kind. Such conclusions and decisions are not to be taken lightly, and you would need more careful procedures to verify their correctness.

Assuming, for example, that the effectiveness of the two treatments will be judged in terms of the percentage of clients using drugs after treatment, it is necessary to decide how large the percentage difference between them must be before it will be considered a real difference. The quality of the answer depends, among other things, on sample size. Indeed, some agencies may find that the sample size required for statistical reliability is larger than the number of clients they have in fact treated. An agency might well find that it could not achieve the accuracy it would demand before accepting an answer to its original research question. In such a case it would be well advised to discard that question and develop easier questions.

The more important the decisions that may be based on the study, the more important it becomes that its findings are scientifically sound. If staff are to be allocated on the basis of study findings, if a request for funds is to be based on them, or if treatment components are to be selected or modified, then the need for rigorous research procedures—a scientific sample, a high completion rate, reliable and valid data, and careful analysis of the data—becomes correspondingly greater.

5. Selecting the Method of Data Gathering

Checking Records

Follow-up studies can be limited to checking your own records, police records, or vital statistics for information they already contain on ex-clients, to see for instance if they have re-entered treatment, been arrested, or died.

Questionnaires

Data can also be collected by mailing questionnaires to ex-clients, but for a number of reasons this will rarely be an acceptable procedure for follow-ups of drug users. As Chapter 5 will show, the mailed questionnaire approach risks exposing to others who may open the mail the fact that the client was a drug user. In addition, the response rate to mail questionnaires is usually low. Finally, there are technical problems in designing questionnaires, and agency staff often will not possess the expertise in questionnaire design that they may have in interviewing.

Interviews

For these reasons interviews will usually be the best choice for data collection. You have a choice between two types of interviews: unstructured ones, in which the interviewer guides the conversation according to a general understanding of the areas in which information is sought; and structured ones, in which fixed questions are asked in a fixed order (with some allowance for skipping questions depending on answers to prior questions). Generally the structured interview will be the preferable choice. The fixed questions it contains may have either a number of fixed responses from which the respondent chooses, or they may be open-ended, with a few lines provided for writing down the respondent's reply verbatim. In Chapter 4 the advantages of each type will be explored further.

6. Selecting a Research Design

Experiments

The agency might elect to design a true experiment, in which clients are randomly assigned to Treatment A and Treatment B, or to treatment and no treatment. The experiment provides the clearest possible answer to research questions because you will know that neither the clients' choice of treatment nor the staff's assignment of individuals to treatment have contributed to your results. But Chapter 3 will show that for ethical and practical reasons the experiment will rarely be a feasible choice--which means it may be impossible to answer some research questions as definitively as you would like.

The Need to Make Comparisons

At the heart of research, whether experimental or not, is comparison--between a treatment group and a control group in the simplest case. A follow-up study which merely describes some ex-clients may be of value in providing the staff with a realistic view of the outcomes which their clients are experiencing, but will tell nothing about the importance of the treatment, or aspects of treatment, in effectuating these outcomes. Effects can only be assessed when comparison is possible. For example, there may be comparisons of two or more treatment groups, comparisons of a treatment group's baseline and outcome measures, or comparisons of the outcome for one agency's clients and those of similar agencies. A wide variety of nonexperimental research designs is available to assist in making such comparisons, and the most common of these are discussed in Chapter 3.

7. Defining the Populations to be Studied

The populations under study should be clearly defined at the outset. For example, if you are interested in whether clients who complete treatment turn out better than those who do not complete treatment, it is important to decide what you mean by "completed treatment." Thus, you may want to specify some minimum time in treatment, say six months or twelve months, as the defining characteristic for that population. Whether it is easy or difficult to define and identify the population will depend to a large extent on how good or how poor the agency records are. Moreover, if a comparison group is to be used in the design, as discussed in Chapter 3, its population also needs to be defined and identified, because a sample for interviewing will be drawn from that group too.

Such a comparison group might be drawn from the population of those persons who entered treatment but dropped out before completing it, or from the population of those who inquired about or applied for treatment, but never entered it. But again, clear definitions will be necessary. For example, how are "inquiries" and "entry into treatment" to be defined? It is also advisable to consider whether all such persons can actually be identified, and unambiguously assigned to one of these categories.

Listing the Populations

In either case, once the target populations have been defined and identified, a list should be made of every individual within them. It is these lists from which the sample will be chosen by random selection.

8. Selecting a Sample and Designing the Interview

If the number of clients who have left your agency is small, you may decide to interview all of them. Usually, however, a sample is chosen. It is much less difficult and expensive to use a sample, and the answers to the research questions can be as definite and as valuable when based on a sample as when based on a total population--"population" here being assumed to be all former clients no longer in treatment.

Chapter 3 will describe in some detail how such a sample can be drawn from the population, using random sampling procedures which are intended to yield a sample which will be representative of the larger population from which it is drawn.

Designing the Interview

Next you will choose the variables referred to above: the dependent, independent and control variables. After that the interview must be designed to measure those variables accurately. The set of directions for giving an interview--including the questions to be asked and instructions about when questions can be omitted--is called the interview schedule. Measurement will be discussed in Chapter 4. Here it is sufficient to note that developing questions that are good measures of your variables is not a one-step procedure. Experienced researchers pretest their questions on several persons (chosen to be as similar as possible to those who will be in the sample). They evaluate the questions for clarity, acceptability, and whether they are really getting at the variables the researchers had in mind. They then revise and pretest the questions again, repeating this process many times before settling on the final form of their questions.

Anticipating the Findings

When designing the interview schedule, you must know ahead of time how the data will be analyzed. One reason is obvious: the variables needed for analysis will not be available unless the interview schedule is designed to obtain them. The researcher should have in mind not only the central comparisons to be made, but also, and more importantly, possible alternative explanations for the findings that may result. As one example, the study may find that post-treatment drug use is less for those clients who have been in treatment longer. It would be tempting to conclude from these results that treatment had worked. But a critic could suggest that those who use drugs less after treatment may have used them less before treatment, and for some reason remained in treatment longer. The researcher could not test the validity of this alternative explanation for the results if the need to have data on drug use prior to treatment had not been anticipated.

9. Estimating Costs

When the detailed research procedures have been drawn up, the planning stage of the project is almost completed. Not quite completed, because it is only at this point that detailed estimates of time and cost can be made. As a practical matter, the decision as to whether or not the research will be done at all depends largely on these estimates. The subject of cost is important enough to be dealt with in a separate section later in this chapter. But it may be noted here that it is not unusual to complete a detailed research design and make a cost estimate based on it, only to decide that the cost is too great. In such a case revisions in the plan may be possible--perhaps simpler and easier research questions, perhaps a smaller sample, or perhaps a shorter interview.

But when a research design is completed and its cost is found to be acceptable, so that a decision to do the follow-up is made, the remaining steps of the project are:

10. Collecting the Data

(See Chapters 5, 6, 7, and 8.)

11. Processing and Analyzing the Data

(See Chapters 9 and 10.)

12. Writing the Research Report

(See Chapter 11.)

Why Have a Written Report?

The need for a written research report cannot be overemphasized. Indeed, the research project is not actually complete until a report is written. Sometimes only a few staff members are involved in the project, and they may be the same staff who will make whatever use is to be made of its findings. Nevertheless, it is not safe to assume that no report is necessary because they already know the results. It is always valuable to have a formal research report, in writing. One reason is that the process of writing clarifies ideas. It is not always easy to describe findings precisely, and specify the limitations that may apply to them, but the very difficulty of the task is an indication of its importance. Inferences from the study may be vague before they are put in writing, and may even differ among staff members, who will never discover the degree of their divergence unless they try to agree on a written draft. Another reason to develop a written report is that it makes possible comparisons between the different parts of the data; it may even uncover contradictions in the data, or inconsistencies which need to be resolved. Finally, of course, the study can make no contribution to scientific knowledge unless and until it is in a form which can be made available to others.

WHO SHOULD CONDUCT THE STUDY

Assuming availability of the necessary resources, programs can elect to conduct the follow-up survey themselves, or they can draw upon evaluation teams available in their communities. There are advantages and disadvantages in both approaches.

Internal Survey Staff

In some studies, it is likely that the existing staff of the agency can do a better job of obtaining the follow-up data from research subjects than could outside experts, providing that the time needed for the follow-up is made available for them. The counselor who has worked with the ex-client may be able to establish contact more easily than a stranger could. Moreover, the counselor's knowledge of the ex-client's history provides a check on the information given at follow-up. This would not be available to a new interviewer hired only for the follow-up.

External Survey Staff

In other studies, external survey staff may be preferred. The decision as to whether a follow-up should be conducted by staff outside or within the program will be shaped by several considerations. One consideration is whether the respondents' answers are likely to be as honest to treatment staff whom they may know and wish to please, as they would to strangers. Another is the potential threat, real or perceived, to treatment staff. Where data are sought simply to see whether modifications in a treatment program may be indicated, no threat of any kind may be involved for the treatment staff who might collect the data. But when the program is to be studied by an outside evaluation group, or when two treatments are being compared, the situation may be quite different. Conceivably one treatment would be discontinued as the result of the follow-up, and jobs would be lost. With so much depending on the findings, it would be unrealistic to expect treatment personnel to be fully objective in data collection.

Subtle Threats to Treatment Staff

Even when no threat of such magnitude is involved, there will inevitably be more subtle threats. Most therapists or counselors are able to sustain the frequent frustrations in their jobs because they are convinced of the value of their work, for at least some of their clients some of the time. It is not easy for them to face the possibility of follow-up findings which might lead to the conclusion that they had been wasting their time for years. They may well have felt that their kind of treatment is preferable to a second kind, and now risk learning that the second was really more effective.

Even if there is no risk at all of deliberate, conscious distortion of findings to support the preferred treatment, there will always be the risk of unconscious efforts to manipulate the findings. This unconscious bias may surface in any of a number of ways: in somewhat stronger efforts to elicit good results from clients who received the preferred treatment, or in stronger efforts to locate the clients from the preferred program who seem most likely to have "stayed clean."

Consider Outside Interviewers

When the data-collector has a stake in the findings, for whatever reason, the question should always be raised whether that person should be used as a data collector. The possibility of using outside interviewers, to whom one set of findings is no more attractive than another set, should clearly be given careful consideration. (Chapter 7 will discuss the advisability of using an interviewer unknown to the client.)

Minimizing the Risks of Subjectivity

If this is not feasible, for budgetary or other reasons, some precautions can be taken to minimize the risks of using staff with a stake in the findings. A fixed interview format will be somewhat more effective in avoiding bias than an unstructured interview, which allows the interviewer great latitude in what he asks. Even then, it should be kept in mind that the interview is never completely safe from bias--the tone of voice, or non-verbal cues, can elicit quite different responses to what seems on paper to be the same question.

Another method of minimizing conscious or unconscious bias in the work of internal staff is to have data collected by staff members who have the least stake in the results. Administrative staff, for example, might be useful in this regard, or counselors might be assigned to follow former clients who were not their own clients.

The Audience As a Factor in Staff Selection

Just as threat to treatment staff is a consideration in the decision whether to use internal or external survey staff, so too is the nature of the audience for which that survey is intended. If the agency executives will base decisions about changes in program on the follow-up findings, they may prefer to have the follow-up done by their own staff. Presumably they know and trust their staff, and it may well be that recommendations from them would, and should, carry more weight than those made by an outside agency.

But if the data are intended for a funding agency or a legislative body, or as a report to a professional journal, the use of agency staff for data collection becomes much more questionable. It cannot be expected that outsiders will have the same confidence in staff objectivity that the agency itself might have. In a report for such an audience it becomes necessary to demonstrate that fairly rigorous research techniques have been used. In general it may be said that if decisions by an outside agency may have an impact on the agency whose program is to be evaluated, it is highly desirable to have the evaluation done by an independent research unit.

Cost Considerations

But the cost of an outside evaluation, or other equally cogent considerations, may rule this possibility out. The choice may be between no evaluation at all, or one done by the agency within its own resources. In this case, in order to have its final report accepted, the agency should adhere to the principles of research design discussed throughout this manual. In the remainder of this chapter, the assumption will be that the agency has decided to conduct its own study; that the study will not be an ongoing one, but rather will involve a single follow-up; and that it will involve clients who have already gone through treatment. However, the more complicated alternatives of ongoing studies, and of prospective studies--in which pre-treatment measures can be designed for the study--will be treated briefly in later chapters.

INITIAL STEPS

The Director

It is advisable that one person be appointed as principal investigator, or director, of the follow-up study. This can be the agency executive, if the study is small enough in scale and s/he has the time to supervise it. But in any case the time required for directing the study should be estimated--generously--and the person selected should be freed, as necessary, from other duties.

Research training and experience are desirable in the investigator, but at a minimum this person should have an interest in research, and a willingness to read and learn from manuals such as this. Administrative skills are essential, especially if the study will involve a number of staff and a sizeable amount of time.

The Staff

If regular staff are employed part-time on the follow-up, there should be a clear understanding among them, their supervisors, and the follow-up director as to how much time they will devote to the project, and at what times. A formal time schedule may be needed. If staff work on the follow-up can be done only on time stolen from their regular duties, some tasks will obviously be executed poorly or not at all.

The Written Plan

It is likely that the appointment of a project director will represent only a tentative decision to do a follow-up study, and to use a specific method for it. At this point the essential research steps described above may have been considered, but only in general terms. If so, the first task of the director--together with any other professional staff who will work on the follow-up--is to produce a written research plan which covers in detail the first six steps described earlier and at least in general terms the remaining steps.

The plan must be written, because only then is it possible to analyze whether all of its parts fit together consistently. Too, it will not be possible to draw up a firm work schedule and budget (see below) until the research plan has been completed in detail. The agency administrator should not give a final go-ahead to the study until the schedule and budget have been considered.

Reviewing Agency Records

Most of the research steps described earlier will be clear enough to guide the project director, who can begin by identifying the study goals and formulating the questions to be answered. It has been noted that these should be stated in terms of variables and relationships among variables. Thus, a careful review of the existing agency records should be made to determine which variables are adequately recorded already, and which will require measurement in the follow-up. The records may also suggest how the study and comparison populations are to be defined, and an early step should be the complete listing of those populations, though the samples should not yet be selected. This preliminary review of records may well suggest some improvements in record-keeping that will facilitate later follow-ups, though not the study now being designed.

Similarly, improvements may be possible in the recording of baseline and treatment variables for future studies. But the first follow-up problem as such will be to locate the persons to be interviewed, and the information needed to locate them should first be sought in the agency records (see Chapter 6 for specific examples). There is also a second type of data to be located in existing agency records, namely, the baseline and treatment variables. Methods for abstracting both types of data from existing agency records will be discussed in greater detail below. It should again be emphasized that the study now designed will have to manage with the data which have been recorded in the past, plus whatever can be measured in the follow-up interview. However, the designers of the study will have a good deal more freedom in planning for outcome variables.

Types of Data to Collect

Follow-up studies of drug users have normally obtained additional data in at least one area, post-treatment use of drugs. Quite often they have also included, as indicators of the effects of treatment, such dependent variables as employment and criminal behavior. Less often, they have looked at such variables as interpersonal relationships, or the individual's satisfaction with life generally.

Defining Treatment Goals

One further preliminary step is advisable at this point in the study. Before an agency is ready to study its own effectiveness, a good deal of time can profitably be spent in discussions of what an agency means by the "efficacy" of a treatment, and how that can be measured.

"Efficacy" presumably refers to the degree to which the goals of treatment have been achieved, so its measurement must eventually be based on the identification and definition of these objectives. Therefore, if assessing the efficacy of treatment is a major goal--and, as mentioned previously, it is one of the most difficult goals to attain--then some thought should be given in advance to the dimensions on which efficacy can be measured.

When the goals of the program have been clearly defined, and the procedures of the study described in some detail, a work schedule and budget should be drawn up (see below). The sample need not be selected yet, but its size should be specified at this point, in order to produce a firm work schedule and budget.

The Pilot Study

The pilot study is a small scale study, on perhaps no more than 10 or 20 ex-clients. It should use the design of the main study, incorporating all the procedures the latter will require. It therefore requires at least a tentative interview schedule. It should involve only clients who will not be in the sample of the main study, but who are as much like them as possible. A good way to ensure this is to select the sample for the main study, and then do the pilot study on others not within the sample but within the same population.

The pilot study gives an opportunity to pretest the interview schedule and to revise questions which prove to be unclear. It very often points to the need to reduce the number of questions in order to keep the total interview time within limits the respondents will accept. Other benefits include the identification of staff who are particularly good at various functions, the development of confidence in the agency's ability to do a good follow-up, and the development of some early cost estimates.

WORK SCHEDULE AND BUDGET

Planning a Timetable

In large scale studies it is essential, and even in the smallest it is desirable, to determine early in the planning stage the extent of the work to be done and to plan and lay out a timetable with target dates for the various phases of the study: definition of the population, selection of the sample, abstracting of data from records, updating of addresses, locating and interviewing of respondents, editing of data, coding, key punching, verifying, analysis, and preparation of the report. It is quite possible that an inexperienced researcher will be inaccurate in making time estimates, but even a timetable that must be modified later will have some value.

The Value of a Timetable

A major advantage of a timetable is that it makes possible an estimate of the staff time needed to complete the project. A timetable will also lead to a listing of all the things that need to be done, and thus open up at an early stage the questions of how they are to be done, highlighting some areas where expert consultation may be needed, or where special training may be advisable for staff who may have to perform functions with which they are not familiar. The timetable will also help to keep the progress of the project on track. At a minimum it will call to the attention of the project director that a problem has arisen, through the delays it causes.

Developing a Budget in Man-Hours

This breakdown of the work to be done, and the approximate time needed to do it, will also be essential in developing a budget for the project. Good follow-up studies will rarely be cheap. They may not require additional funds, but by diverting staff time from other duties they do involve hidden costs which must be evaluated by the agency administrator before approving the project. Inexperienced researchers do not always fully appreciate that several man-years of work may be required in even a small project. The administrator should determine in advance that these can be spared, and calculate how the normal duties of staff will be covered during the time they spend on the project. Such considerations may lead, in some cases, to a decision that the agency cannot afford to do the project, or that it must be scaled down in size. But this would be better than to invest a great deal of thought and many months of work in an overly ambitious study which later might have to be abandoned or drastically scaled down in such a way as to greatly reduce the usefulness of the results.

Costs Associated with Telephone Follow-Ups

Follow-up studies can vary so widely in aims, methods and scope that it is difficult to be specific about costs. The following general suggestions can be made, based on the experience of many researchers.

Telephone follow-ups are generally quick and inexpensive. The cost of leasing an extra line or of purchasing more extensive service can be explored. The speed and cheapness of such a study, however, will be bought at a high price in terms of its scientific value. Those ex-clients who do not have phones will be lost from the sample, probably biasing its findings. In addition, telephone interviews must usually be shorter and simpler than face-to-face interviews, so there is a cost in lost information. Finally, as Chapter 5 will discuss, it can be so difficult to maintain confidentiality of information when the telephone is used that ethical considerations may preclude this approach completely.

Costs Associated with Mail Questionnaires

Mail questionnaires are also relatively inexpensive. Again, however, there is a serious question of maintaining confidentiality if mail is used. There are also technical problems in questionnaire construction for which consultation may be needed. It is normally necessary to use repeat mailings--at least once, often two or three times--to those who do not return the questionnaire. Each of these repeat mailings must contain copies of the questionnaire, since the first may have been lost or thrown away. It will therefore be necessary to print two or three questionnaires per person in the sample, and it will normally be cheaper to have these all printed at the start.

Mail questionnaires also involve added clerical costs to keep track of which questionnaires have been returned, and to mail the second and subsequent waves to those who have not returned the questionnaire. Stamped return envelopes will be needed with each mailing. Since several weeks have to be allowed for replies after each mailing, the completion of a mail survey could take several months.

The most common problem with mailed questionnaires is a low return rate. No matter how good the sample may be, it will be impossible to draw statistically justified inferences if the return rate is low. For this reason a mail questionnaire will usually be a poor choice of method unless there is a way to find and interview at least a sample of those who do not return questionnaires, or unless the research question is one that need not be answered with great accuracy.

Costs Associated with Personal Interviews

Face-to-face interviewing has several advantages: it facilitates confidentiality; it assures that the right person is answering; it permits more questions, and more complex questions; and it permits probes where answers are unclear. But it is the most costly research method in terms of time and money.

In a very large scale follow-up, in which 500 or more ex-clients are to be interviewed over a large geographical area, and some 20 or more interviewers are to be employed to do the interviews, the agency probably would be wise to contract the entire project out to a professional research organization. The problems of recruiting, training and supervising the interviewers would simply be too great for an agency which did not have full-time staff experienced in these functions. Even for smaller studies, in which several hundred ex-clients are to be followed, a contract with an experienced research organization should be considered: extra interviewers would probably have to be employed anyway in such a case.

However, for the remainder of this chapter it will be assumed that the sample will be 300 or less in size, and that interviews are to be done by existing staff of the agency, perhaps supplemented by one or two extra persons brought in to help temporarily. Under such circumstances the major cost to the agency will be the time its own staff will devote to the follow-up. A good rule of thumb is that everything will require considerably more time than one would expect.

Costs Associated with Locating Respondents

Based on the experience of previous researchers, the time required for the interview itself will be much less than the time it takes to locate the ex-clients in the sample, and to arrange for the interview. (Chapter 6 discusses in some detail the problems in locating respondents). Only the pilot study mentioned earlier will provide good estimates of these time requirements. In addition, interviews done in the city where an agency is located will provide estimates of how large a percentage have remained in the city, and how long it takes to locate and interview them. It will obviously take much longer, and cost more in travel expenses, to interview those respondents who have moved to other cities.

A very rough estimate, which could vary widely in different situations, is that it could take one man-year to locate and interview a sample of 200 ex-clients, assuming these were followed to wherever they might happen to be, and assuming that a completion rate of 85 per cent or more is sought. The time requirement would be less if the follow-up were done soon after the completion of treatment, since fewer would have moved. It should be noted that the estimated man-year could not be collapsed into one month by using a dozen interviewers: there will inevitably be delays in finding leads to ex-clients, and there will always be some whom it will take months to locate.

Travel Costs

Travel costs can be an important element in the overall cost estimates. If they are too burdensome, you may need to consider redefining the population to be studied, and therefore the population about which inferences can be drawn from follow-up findings. For example, the study may need to be confined to those ex-clients who have remained in the city, or within a fairly short distance of the city.

Clerical Costs

It should also be recognized that an appreciable amount of clerical time, and time of the study director, will be needed to maintain records on the steps taken to trace the persons in the sample and on the attempts made to interview them, as well as to process the data obtained in interviews.

Chart 1 outlines the various cost items which, at a minimum, should be considered in constructing a budget. When the best possible estimates of time and money costs have been made, the project director and the agency administrator should realize that they are only estimates, and that unforeseen contingencies may increase the costs. The study should not be approved unless it is well within the agency's resources.

Adjusting the Study to Resources Available

The costs, of course, must be judged in light of the value of expected findings. It is at this point that an agency may discover that the questions it most wants to answer--"How good is our treatment program? ", "How much are our clients helped? "--cannot be answered as a practical matter because the research design needed for such questions cannot be approximated, or because the costs are far too great, or because the needed time cannot be spared from normal duties. Such a conclusion is, of course, disappointing. But it is far better to choose a different question to ask, or to use a smaller sample and thus accept a less precise answer, than to go ahead with an unrealistic proposal in the hope that some miracle will occur. Consultation can be especially helpful at the point when such decisions must be made (see below).

CHART 1

Research Costs To Be Considered

Personnel Costs

- Research investigator(s) - general
- Any additional staff time required for:
  - Coding answers
  - Keypunching answers
  - Data processing
  - Search of agency records (if any)
- Interviewers
- Secretarial support
- Consultants

Non-Personnel Costs

- Interviewer travel costs
- Printing or duplicating
  - Questionnaire or interviews
  - Postcards, return envelopes, etc. (if any)
  - Final report
  - Xeroxing
- Postage
- Telephone
- Computer Time
- Payment to Respondents (if any)
- Office Supplies

ADMINISTRATIVE AND RESEARCH RECORDS

Assuming that the research plan has been approved, the next steps to be taken by the follow-up director can be examined in terms of the records needed for the efficient conduct of the study. These records need not be extensive, but a few will be essential.

The Lists of the Populations for Study

The first requirement is a complete list of all those clients who were eligible to be selected for the sample, with a written description of how eligibility was defined. The same kind of list is needed for control or comparison groups if these are used. Chapter 3 will discuss some of the ways in which a sample can be selected from a population. A clear record should also be kept of exactly how each sample was drawn from its population.

The Control File

Once the sample has been selected, the first record to be established is a card or folder for each individual within the sample selected. This will serve as a master control file.

In any follow-up study, as mentioned earlier, the existing agency records of the ex-clients selected for the sample are examined for any information which will help to locate the respondent, and to confirm his identity as the correct person to be interviewed--items like addresses, birthdate, nicknames or aliases, social security number, names and addresses of relatives and friends who might know his current location, and any other descriptive information. These data should be recorded on a "search sheet" to be used by the persons who attempt to locate the subject.

The purpose of the control file is to reveal at a glance what has been done and what remains to be done to complete the follow-up. At a minimum, the control file should record the following: (1) what has been done to trace the respondent, the leads that have been exhausted and the new leads that have been found; (2) the efforts to interview him, and their results; (3) whether an interview has been completed; (4) whether the interview has been edited for completeness and clarity, including any return of a case to the interviewer for further contacts to get missing information; (5) whether the coding of data has been completed; (6) whether coding has been checked for accuracy; and (7) whether the data have been transferred to IBM cards or the equivalent for entry into the data bank for analysis. Similar steps in retrieving and coding information from treatment records and information from police records or vital statistics should be listed and checked off on the control card.

The control form should provide space for dates to indicate when each step is completed, and space to describe the reasons for any difficulties and what is to be done about them. In general, the simpler the form the better, but it must contain enough information so that the project director can quickly review the status of all cases. This will make possible periodic checks to see if the project timetable is being met, and to identify any problem areas that may need special attention.

The Form for Recording Baseline and Treatment Variables

As mentioned previously, another kind of data to be abstracted from the agency records consists of the baseline and treatment variables that will be used in the data analysis stage. If these are very few in number, they can be included on the control card; but normally it will be best to design a separate form for recording these variables. They should never be entered on the interview schedule, because they should not be available to the interviewer at the time of interview.

Records of the Decision-Making Process

Research events and decisions should be recorded in writing, as they occur, throughout the entire research process. There are literally scores of problems to be faced and decisions to be made during the research process, and it is amazing how quickly you can forget which decisions were made, and why. Since some procedures may need to be repeated later, and since all should be in the final report, a careful record of each of these decisions should be preserved.

There is a second, less obvious, reason for recording the early decision-making about any research project. This is that the researchers begin a project with many questions in mind, and gradually formulate answers to them as the data come in. They become so familiar with the project that the answers seem obvious and the questions seem hardly worth mentioning when the report is written. But the original questions remain just as interesting to the rest of the world, who have not seen the data come in. The researchers can use their early written records to recapture that simpler level at which the written research report should begin.

USE OF CONSULTANTS

Consultation with "experts" may be useful at almost any stage in the research process. It comes close to being essential at one point, when the research plan has been developed and committed to writing, and the agency administrator is about to decide whether or not to approve it. A consultant can be helpful at this point in at least three ways:

Technical Consultation

(1) To review the plan from a technical research viewpoint. If there are any serious flaws in the design, this is the time to find them. A consultant may also be able to suggest less expensive or more efficient ways of handling some problems.

Legal Consultation

(2) To review the plan for legal and ethical problems. A follow-up study is research on human subjects, and automatically involves risks for those subjects in that damaging information about them may be divulged. The legal and ethical requirements for such research have changed rapidly in recent years. Unless an agency is familiar with these requirements, and has existing institutional

arrangements to meet them, it is not merely desirable, but essential that it secure consultation in this area.

Help with Request for Proposal

(3) To assist in preparing a Request For Proposal. If contracting with a research organization to do the follow-up is under consideration, consultation on preparing a Request For Proposal is indicated. This need not be discussed in detail, since the assumption here has been that the agency will not choose this option. If it did do so, it should bear in mind that there would be problems in preparing a technically adequate RFP, and in evaluating the ability of potential contractors to find hard-to-locate subjects. The cost of consultation would be minor by comparison with the cost of such a contract.

What Kind of Consultants?

But an agency administrator may well ask: what kind of experts are needed, where can they be found, and how much will they cost?

Few researchers have conducted follow-up studies of drug users to date, but the number is growing. There are more who have directed follow-up studies of other populations, including alcoholics, persons with mental health problems and persons treated for physical illnesses. The experience of such researchers will be directly relevant, with some allowance for their lack of familiarity with drug users and the drug subculture. There are many more who have never done follow-up studies, but are experts in research design. Such potential consultants are to be found in the professional institutes for survey research, in several agencies at all levels of government, and in universities, particularly in their medical schools and social science departments.

Finding Consultants

One possible source of help, either through its own staff or through its knowledge of others with relevant experience, is the Single State Agency. Indeed, advising them of the study may be desirable for another reason: in at least one state, the Single State Agency must approve the follow-up before it may be undertaken. The funding agency may also be a source of help. In addition, the National Institute on Drug Abuse, and staff in the Regional Offices of the Department of H. E. W., while limited in the direct staff help they can provide, may be able to identify local sources of consultation.

The Cost of Consultation

Consultation costs money, but not necessarily very much. For only a few hundred dollars--perhaps less if no travel is involved--an agency can buy a day or two of a consultant's time. This should be done only after the agency has completed most of its own planning so that it can raise specific questions and give the consultant something concrete to react to. To go to a consultant with a general question, like "We would like to do a follow-up study. How do we do it?", would be a waste of time for both parties. But if the agency has used a manual like this one, and has done its homework before approaching a consultant, it can expect to get real help.

Another point in the research cycle at which a consultant may be of particular value is at the reporting stage. Once a first draft of the final report is completed, it may prove useful to get the opinion of an experienced researcher regarding the analyses and deductions it contains.

Chapter 3

SELECTING A STUDY DESIGN

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Designing the Study

Designing a research study is perhaps the most critical step in the research process. Without a good design, the results of a study will be hard to interpret and its conclusion less than convincing. For these reasons, careful statements not only of the study design but also of the sampling design are prerequisites to actual data gathering. Such statements are also a prerequisite to funding applications and should be available to consultants employed to help with the study. Clearly, faulty design features are easier to correct at an early stage than after the data have been gathered and analysis has begun.

Designing any research study requires careful advance planning. The first step is to specify what that study is about. On the surface this seems easy, but in practice it is often difficult. It is necessary to specify at the beginning of a study precisely what questions are to be addressed, and to design the research in such a way that it will provide answers to these questions.

This manual is addressed primarily to those interested in reducing clients' drug use and the negative consequences of that use. Studying other goals of treatment agencies, such as changing community attitudes toward drug users or reducing crime among users not in treatment, would require different research designs. Further discussion of designs aimed at such goals will be found in Appendix A.

STUDY DESIGNS

Assuming that a clear specification of the study's objectives has been made, as discussed in Chapter 2, the next step is to choose the research design most appropriate for achieving these objectives. In almost every case, developing a research design will require a number of compromises calculated to strike a balance between what those doing the study would like to learn and what the available resources will enable them to do. Since no one study design will answer all the questions that programs typically will want to pursue, the design chosen should deal with the more important questions while making the most efficient expenditure of research effort.

This discussion presents a number of common designs and shows the kinds of questions each can answer. It begins with the experimental design, wherein experimental and control groups have been selected at random, and it ends with some comments on what is perhaps the simplest design of all--the case study. In general, it may be said that the more a design resembles an experiment, the greater the confidence one can have in the study's results. Such studies, however, require advance planning, and may raise difficulties of an ethical, legal, and technical nature. Therefore, it is unlikely that many programs will be able to adopt one of the earlier designs discussed here, particularly the true experimental design. However, it is useful to be familiar with these designs in order to better understand the strengths and limitations of the later ones.

THE EXPERIMENTAL DESIGN

Random Assignment of Subjects to Experimental and Control Groups

The experimental study design, which includes experimental and control groups chosen at random, is a powerful one. The investigator randomly assigns each subject to either the experimental or control group. Random assignment means that each subject has the same probability of being picked for either group.

Introduction of Experimental Condition

After assigning the subjects to experimental and control groups (by flipping a coin or using a random number table), the investigator next measures specific characteristics of the subjects, such as age, sex, and drug experience. The final step in the experiment is that the investigator introduces a unique experimental condition--such as a specific treatment--to the experimental group of subjects but not to the control group. By measuring the differences between the pre- and post-experimental characteristics of the experimental and control subjects, the investigator can ascertain the effects of the experimental condition with great precision. This can be done because all other effects have been randomly distributed between the two groups.

Uses of the Experiment

The classical experiment is best suited for studies in which the researcher has specific, easily definable, and usually rather limited research questions to test, and in which a great deal of control over the research procedures can be exercised. Uses of this method in a drug treatment clinic might include comparing the effectiveness of different methadone dosage levels, or of the program's frequency of taking urine samples. However, the experimental design has some limitations for testing the effectiveness of a whole treatment modality or the relative effectiveness of one modality as against another. This is true because treatment modalities are actually made up of many treatment variables. If one treatment modality is found to be better than the other, the researcher may not know which aspects of the better treatment experience made the difference.

For example, it may be the case that a charismatic director of one treatment program was responsible for its unusual success. Obviously other attempts to replicate that program may not have such a director and therefore may not experience its success. Similarly, it could be misleading if an experiment disclosed that a treatment modality had been ineffective as a whole: some elements of the program might have been very effective, but these may have been offset by other counterproductive elements. Thus, unless the treatment variable is specific and can be isolated from other treatment variables, it is probably not a good candidate for a true experiment.

Practical and Ethical Problems

The classical experimental design is also ill-suited for exploratory studies or for studies that seek information on a large number of in-treatment variables. Since the classical experimental design can handle only one treatment variable at a time (or a very limited number of such variables), it is not an efficient design for those who wish to explore numerous aspects of their treatment programs concurrently. For this kind of study, one of the nonexperimental designs discussed below would probably be more useful even though these designs will yield results which are less conclusive than the results obtainable from a true experiment. The experiment also presents some practical problems. It requires random assignment of clients, or prospective clients, to treatment and no-treatment groups. Ethical, legal, and practical restraints will rule this out in many programs.

NON-EXPERIMENTAL STUDY DESIGNS

Before-After Studies with Comparison Groups

The research design closest to the classical experiment which most programs can realistically consider is the before-after study with comparison groups. This design differs from the experiment in that subjects are not assigned at random to experimental and control groups. In the before-after study, comparison groups are used instead of randomly selected control groups. As one example in the drug follow-up context, the subjects for study might consist of those ex-clients who received a particular kind of treatment, while the comparison groups might consist of clients who received another kind of treatment, or of persons who never entered treatment at all.

Similar to the classical experiment, this design calls for before-after measurement. That is, specific characteristics of the experimental and comparison group subjects are measured before and after the experimental subjects have been exposed to the experimental variable. If these pre-experiment characteristics have changed among the experimental but not the comparison group subjects, then the change may be attributed to the experimental condition.

However, without random assignment to experimental and control groups, this interpretation must be tentative. For example, if the experimental group subjects exhibit a greater reduction in drug use than the comparison group, this may be attributable to the experimental subjects rather than to the effects of treatment. Even though experimental and comparison group subjects may have had comparable drug use rates at the beginning of the study, it is possible that they could have differed on other dimensions--which could have led to the more favorable results for the one group. (Many of these differences may even be unknown to the researcher.) Unless these differences are randomized between the two groups, as in the true experiment, you can never be sure that observed differences are due only to the treatment condition.

Choosing the Comparison Groups

For this reason, it is important to choose comparison groups which are as much like the experimental group as possible. In a study of treatment effectiveness, comparison groups might include clients who left treatment early, who applied for treatment but never actually received it, or who are on a treatment waiting list. In each of these cases, those in the comparison group are similar in that both made application for treatment. But they are not exactly alike. For example, people who apply for treatment but never receive it may be in more serious legal straits than those who actually enter treatment (perhaps they went to prison rather than treatment). Moreover, those who leave treatment early may be less well-adjusted psychologically than those who complete treatment, while those on a waiting list may be less heavily involved with drugs than those actually admitted to treatment. These examples suggest some of the reasons why comparison groups can only approximate experimental control groups. As a result, research findings based on differences between comparison groups and treatment groups should always be judged cautiously.

One way to enhance the reliability of the findings is to employ more than one comparison group. If it develops that a number of comparison groups exhibit no change during the course of the study but that the experimental group does show a change, you can more confidently attribute the results of the study to the effects of the experimental condition. For example, if program applicants, those on a waiting list, and those who left treatment early all showed less improvement than those who finished treatment, you might be justified in concluding that something about finishing treatment was having a positive effect. However, this might be a false conclusion if program personnel are admitting those who seem most likely to succeed in the first place.

Limitations of the Before-After Design

It may be concluded that the before-after design using comparison groups shares some problems with the classical experiment while having additional problems of its own. Like the classical experiment, it has limitations for studying "whole" programs when only one or two programs are included in the study. In addition, it suffers because the comparison and experimental subjects are not randomly assigned. (The limitations of non-random selection of clients into the various groups being compared are discussed further in Chapter 10.) Nevertheless, this design does avoid some of the ethical, legal, and logistical problems associated with random assignment, and for that reason it will probably be a more feasible study design for many programs.

One point deserves re-emphasis: the before-after design with comparison groups, like the classical experiment, calls for multiple measurements: both the experimental and the control subjects must be measured twice--once before the experimental group enters treatment, and again after the experimental group has finished treatment. Unfortunately, such prior measurement is not always possible in follow-up studies. Most programs have intake data on all clients which were gathered at the time of program admission, but these data are not always sufficient for reliable research analysis.

There are at least two reasons why such data may be insufficient. First, intake interviews and clinical records often include a great deal of information which is of little use to researchers later on, while excluding important research variables needed for later analysis of treatment impact. Second, intake data are often gathered by different persons and under conditions quite different from those prevailing later when a research staff gathers the "after" measure ment data. In consequence, routine clinical records do not often include the data needed for a before-after analysis. Therefore, any program which is contemplating a follow-up study with a before-after design using comparison groups should consider the problems of research data gathering well in advance of the study. Indeed, such planning should ideally begin before the first patient who will be included in the study even applies for treatment.

Panel Studies

Take away the control and comparison groups from the two designs discussed previously and you are left with what is called a panel design. In this design, a group of subjects is studied before and after exposure to an experimental condition, and assessments of experimental impact are limited to these before-after measurements. An example of a panel study in a treatment program would be the relative prevalence among clients, prior to and following treatment, of illegal drug use or arrests.

Problems in Interpreting Results

The major disadvantage of the panel design stems from the fact that people tend to change with time even when there has been no treatment intervention. Without a control or comparison group it is never clear that changes observed in an experimental group were the result of the experimental condition; they might have occurred anyway. This problem of interpreting the results of a panel study has been discussed in a number of studies. It is particularly well illustrated by the investigations of Walter Miller in Boston (Miller, 1959, 1962).

Miller's research explored the question whether detached gang workers could reduce the amount of delinquent conduct among boys in delinquent gangs. The early results of the study indicated that they could; the rate of delinquency among gang members went down following the assignment of a worker to the gang. However, this observation was made without benefit of a comparison group. When comparison groups were added to the study design, namely, gangs from the same community without workers, it was found that rates of delinquency declined for these groups as well. What the study had originally found, then, was mistakenly attributed to the presence of a worker. Evidently the true causality was that delinquent boys in general tend to abandon their delinquent involvement as they grow older. Since the criminal involvement of drug users may also decline simply as a function of maturation, comparison groups become very important in drug treatment follow-up studies.

Uses of the Panel Design

For the same reason--absence of comparison groups--the panel design is not a good design for assessing the overall effectiveness of a treatment program. Nevertheless, it is a useful design for some kinds of studies. In particular, it is a convenient design for studying variations in individual client responses to treatment. For example, clients who have done well can be compared with those who have done poorly to see if there are discernible baseline differences between the two groups. If such differences are found, treatment programs can then specify the populations for which particular treatments are most successful.

After-Only Studies

When a program finds for one reason or another that it cannot gather pre-treatment data for a follow-up study, it will have to choose an after-only study design. There are two main kinds of after-only design: one with comparison groups, and one without. Follow-up studies employing an after-only design without comparison groups are numerous. Two such studies are discussed here. However, the problems of interpreting the results of after-only studies without comparison groups are very severe. It is therefore useful to discuss this particular design in conjunction with the after-only design which uses comparison groups--as illustrated in the third study discussed here.

> *The Vaillant studies.* Of the studies employing an after-only design without comparison groups, the studies by Vaillant (1966, 1973) are as well known as any. These reported the progress of a group of clients who had been treated at the National Public Health Service Hospital at Lexington, Kentucky. The key to the design of these and similar studies is the follow-up, some time after discharge, of a particular group of clients who have received treatment for drug abuse. After leaving treatment, the groups of clients studied returned in large proportions to drug use and other forms of crime, or in other ways exhibited undesirable behavior or social adaptation, such as by alcoholism and unemployment. The conclusion most often drawn was that the treatment experience had not had a significant, lasting, positive effect on these populations of drug abusers.

The point to be kept in mind is that this conclusion could have been wrong. It is altogether possible that these former clients might have done even worse if they had not received treatment. Without a comparison group, this possibility cannot confidently be eliminated.

This example points up the fact that the absence of a comparison group is the major deficiency of many after-only studies. This opens the door to misinterpretations of the data, as illustrated by the findings of two recent follow-up studies of clients who had completed methadone maintenance treatment (Riordan, Mezritz, Slobetz, & Kleber, 1976; Gould, Forrest, & Kleber, 1975). The study by Riordan et al. also used an after-only design without a comparison group. However, the Gould et al. study used an after-only design with comparison groups.

> *The Riordan et al. study.* This study included follow-up data on 38 individuals who had successfully completed detoxification from methadone maintenance in good standing. It reported the following results: 68 per cent had remained drug free, 82 per cent were employed, 21 per cent were on probation or parole, and 26 per cent had been arrested. These are very encouraging results; they contrast markedly with the results of earlier follow-up studies.

However, as with the Vaillant studies, the question arises whether it can safely be concluded that these findings were the results of treatment. To a large extent, it cannot. This does not become obvious until the follow-up results of those who completed treatment are compared with the results of other groups of clients who did not receive treatment or did not finish it.

> *The Gould, Forrest, Kieber study.* Table 1 presents follow-up data from the second study on 30 clients who had completed methadone treatment. These same 30 clients were included among the 38 in the former study by Riordan et al. But Table 1 presents additional data on 93 clients comprising three comparison groups: (1) clients who left the program before completing treatment; (2) clients still in treatment; and (3) people who applied for treatment but never entered it. (All of those in this latter comparison group were eligible for methadone maintenance in terms of age, type of drug use, and years of prior drug use.)

The data in Table 1 show that those who completed methadone treatment fared better, by and large, than those in the comparison groups. However, the differences between the treatment and comparison groups are surprisingly small and in some cases not statistically significant. What is particularly interesting is how well those who never received treatment or those who left treatment early were doing. They were doing far better than anyone had expected and better than could have been predicted from the results of previous follow-up studies of clients who had received treatment (Vaillant, 1966, 1973), or of clients who had left methadone treatment against medical advice (Perkins & Bloch, 1971).

In fact, the comparison of these other groups with those who finished treatment leads to the conclusion that treatment alone explains only part of the somewhat more favorable condition of those methadone clients who completed treatment. This conclusion is even more compelling in light of some other factors not disclosed in Table 1: namely, that many of those who completed treatment were employed by drug treatment programs, accounting for their employment advantage, while evidently many of those who left treatment early, or never entered it, did so because they went to jail. Overall, then, it is possible to conclude that methadone treatment probably had some positive effects in this case, but that these effects were modest.

Limitations of After-Only Studies

The after-only design without comparison groups, then, is not a good design for program follow-up because the results of studies based on this design are ambiguous. With comparison groups, the after-only design yields results which are somewhat less ambiguous. However, as with the before-after design using comparison groups, there is still considerable room for misinterpretation. This is particularly true if there were major differences between the experimental and comparison groups even before treatment.

Advantages of After-Only Studies

Nevertheless, after-only studies do have one very important advantage over other designs: they can be done after the fact. This advantage will be especially important for programs which have operated for some time without planning for some kind of before-after study. The after-only design will offer programs a

TABLE 1

A Comparison, at Time of Follow-up, Of Three Groups of Methadone Treatment Patients And One Group Which Received No Treatment (Adapted from: Gould, Forrest & Kleber, 1975)

	Methadone Patients			Non-Patients
Characteristic	Completed treatment	Left treatment early	Still in treatment	Never entered treatment
Employed	77%	41%	52%	54%
Incarcerated	0%	22%	0%	26%
Criminal cases pending	3%	22%	4%	11%
Uses heroin	0%	16%	20%	23%
Number of persons in the study	30	32	25	36

Note: The percentages within each column are not mutually exclusive.

great deal of useful information which otherwise would have been lost to them, particularly if carefully selected comparison groups are used. After-only studies are useful tools in program follow-up, as long as it is remembered that the results should be considered cautiously.

Case Studies

Case studies are studies of a single ex-client, program, or community. These are by far the simplest studies to design, but they are the least powerful instrument for obtaining information from which conclusions of more general applicability may be drawn. Their value lies largely in their ability to generate hypotheses, which can be tested in more systematic studies. Therefore, this technique will not receive any extended discussion here.

SAMPLING

A case study is a sample of one. By contrast, a study which gathers data on every unit of analysis about which generalizations are to be made is called a complete enumeration. For example, a study of the latter type might gather data on all clients who have applied to a particular clinic for treatment. The listing of total client populations discussed in Chapter 2 would provide the basis for a complete enumeration study. In between these two extremes fall those studies which include more than one case but less than all cases. Care needs to be given in such studies not only to study design, but to sampling design as well.

The weaknesses of case studies have already been mentioned. On the other hand, studying "everyone" is not always the best alternative. If a program has many clients, studying every one of them might be very expensive indeed. Expending the resources necessary to locate and interview large numbers of persons diminishes the resources which could otherwise be devoted to a more careful and detailed study of fewer persons and to securing interviews from a higher proportion of those selected.

There is no virtue per se in studies with large numbers of subjects. What is important is that there be sufficient numbers of subjects to make statistical generalizations, with acceptable margins of sampling error. In some simple cases this may take no more than a hundred subjects. In more complicated situations, proper sampling procedures will call for larger sample sizes.

The Purpose of Sampling

The purpose of sampling is economy. By using a sample, researchers are able to develop information about a large population while only gathering data on a proportion of the subjects in that population. To do this successfully, however, it is crucial that the sample be drawn from the larger population at random. In a truly random sample, every member of the larger population has an equal probability of being included in the sample. Samples drawn for convenience only are not random samples in the mathematical sense and thus cannot be used to make statistical generalizations about an entire population. For example, if the sample includes only those clients who are easy to locate or willing to give an interview, there is a good chance that their outcomes will not be the same as the outcomes of the whole population of clients. Systematic samples, consisting for example of every seventh name in the agency files, should also be avoided because they are not random. Since true random samples are not difficult to get, researchers should not be satisfied with less.

Random Number Tables

To assist researchers in sample selection, random number tables are available, and their use is recommended. Such a table would be similar in appearance to the following example:

(Columns)

(Rows)	1 - 4	5 - 8	9 - 12	13 - 16	17 - 20	21 - 24	25 - 28
1	13 79	71 62	07 80	30 01	80 79	31 50	43 80
2	95 61	78 24	39 95	27 38	10 95	84 57	51 32
3	47 53	23 05	30 61	08 87	45 50	94 70	94 73

Almost any social science statistics text book will include such tables of random numbers, as well as the details necessary for drawing a random sample from a population. (See Appendix B for a convenient example of a random number table.) The method described here for drawing a random sample is not the only such method, but it is theoretically correct and is generally accepted by most experienced researchers.

Preparing to Use the Table

How do researchers use a table of random numbers in drawing a sample? Chapter 2 of this manual discussed the advisability of making a list of every individual within the populations to be studied. Such a list might be in alphabetical order, but this is not essential. Each individual on the list should be assigned a number in series, commencing 001, 002, 003, and so on, using enough zeros so that all numbers on the list have the same number of digits. Assume for the sake of example that the total list of the experimental and comparison group population contains 624 names, and that you wish to draw a sample of 200 names from this list.

Using the Table

To use a number table to draw such samples you would be working with three digit numbers on the number table, since the numbers on your lists involve a maximum of three digits. You would start on the number table at the beginning (or even better at a point chosen at random). The numbers on the table are usually read like a book, left to right from row to row, but they may also be read from top to bottom or from right to left.

In the table used as an example above, the first group of three random numbers is 137 (row 1, columns 1-3). The person on the list assigned that number would be taken from the population list and entered on the sample list. The next group of three random numbers is 971 (row 1, columns 4-6). No number on the population list has this number, so it should be discarded. (Numbers can also be discarded if duplicates happen to come up.) The next group of three random numbers is 620 (row 1, columns 7-9). The person on the list with that number would also be entered on the sample list. Following this same basic procedure, the table is then used until the desired 200 names for each sample have been obtained.

This brief and simplified explanation of how to use such tables to draw a sample has been adapted from two standard textbooks on statistics (Arkin & Colton, 1963; Guilford, 1965). You may find it useful to consult such textbooks for further help in drawing a sample. For a more detailed discussion of sampling design, consult Sudman (1976).

As noted earlier, if an experimental study design is employed, then subjects will be randomly assigned to experimental and control groups from a master list of clients. If the study involves one of the other designs discussed previously, then a random number table will be consulted to determine which persons from the treatment and comparison group populations will be chosen for interviewing and other data gathering efforts.

The Simple Random Sample

There are also different kinds of random samples. The most straightforward, described above, is called a simple random sample. You would begin with the master list of a population--for example, a list of all persons who had applied for treatment to a program during a specified period of time. From this list, a sample of the desired number of subjects would be chosen by use of a random number table. Once the sample is drawn, the subjects within the sample are then classified into subsamples. For example, all those subjects within the sample who had completed treatment would constitute the subsample designated as the experimental group. Other subsamples might consist of those subjects within the sample who did not enter treatment, who left treatment early, and who were still in treatment. These three subsamples would constitute comparison groups.

A simple random sample is the easiest to obtain and the most convenient for later statistical analysis. However, it will not always be the most economical procedure where the number of clients who completed treatment is markedly greater or smaller than the number of subjects within the various comparison groups.

For this reason, among others, statisticians have developed another kind of random sample called the stratified random sample.

The Stratified Random Sample

In using this procedure the study population is stratified into homogeneous subpopulations, for example into treatment and comparison populations, before the samples are drawn. Sampling from predefined subpopulations gives the researcher better control over the proportion of the respondents which will come from the various subgroups of interest.

For example, the researcher may choose to draw samples of equal size from each experimental and comparison group population, regardless of what proportion of the total client population is actually in each group. (It may be that having equal sized groups would be optimal for analysis.) What is given up by this procedure is the ability to combine the subsamples in a simple manner for an overall statistical analysis of the total population. However, corrective reweighting may not be too difficult.

Sample Size and Confidence Limits*

If the number of clients from your program eligible for inclusion in the study is relatively small, then you will probably want to include them all in your follow-up. If the number is large, say more than 200, resource constraints may require that you limit the study sample to a subset of the eligible clients.

Determining the appropriate number to sample can be a somewhat complicated procedure. A rough rule of thumb is that you should try to have a minimum of thirty actual respondents in each group which will be compared to other groups. However, there is nothing magical about that number. The following discussion shows how a larger number will help to improve the certainty of your conclusions.

You can know ahead of time just how certain you will be about any statement you can make for a group of a particular size by looking up in a table the confidence limits of any proportion or per cent. For instance, if you have a random sample of 40 clients who were in a job-training program, and 80 per cent of them have a job at follow-up, you can state with almost complete certainty that among all those who had job-training, at least 64 per cent had jobs, and not more than 91 per cent had jobs. (Statistical tables giving the 95 per cent level of confidence were used for these computations). If that is as accurate as you need to be, the sample is big enough. On the other hand, if you feel your estimate must not be off by more than 5 per cent (i.e., the true values must be between 75 per cent and 85 per cent) you will need a sample of 300, instead of 40.

The total sample size needed is often determined not by how accurate a single estimate must be, but by how small a difference between percentages in two parts of the sample you want to be able to show as statistically significant (i.e, that you want to be sure did not occur by chance).

Suppose you are comparing drug recidivism in a treatment group which received group therapy to the recidivism rate in a treatment group which had not received that therapy. Perhaps you expect to find that 70 per cent of the group-therapy clients stayed off drugs for a year, while only 40 per cent of the comparison group had done so. You then will want to know how you can be sure that the group-therapy group did not just happen to come out better due to random sampling variation, when really there would be no differences in success between the two groups if you were able to include all potential clients in your sample. In other words, you want to know what the chance is of getting an observed difference as large as you did because by chance your sample happened to be unrepresentative, when in fact there may really be no difference.

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*This subsection was written by Lee Robins and Lloyd Johnston.

The larger the samples on which the comparison is based, the less the chance that the results occurred because of chance or "sampling error." In fact, you can determine from Appendix C exactly how big a sample would be required for you to be 95 per cent certain that a difference that big did not occur by chance, but reflects the fact that group therapy is at least somewhat more successful (though not necessarily 30 per cent more successful). The table shows the sample sizes needed, assuming equal-sized groups, for you to be able to say that the observed difference in the proportions had no more than a five per cent chance of occurring due to sampling error. In this case, the table indicates that to draw such a conclusion, you would need 45 cases each in the treatment group and comparison group (this number has been circled in Appendix C). To put it into statistical terminology, you would need 45 cases in each sample group to say that there is "a significant difference between the two groups at the 95 per cent confidence level."

The table in Appendix C makes clear that very large samples are required to show statistical significance when differences are small. But if samples are huge, even a one per cent difference would be statistically significant, even if it would be of little interest practically.

If you are more concerned about determining the size of the difference between two rates, rather than simply knowing whether there is a difference, that is also possible to estimate. Appendix D describes the procedures to be followed in determining the sample sizes needed to make such determinations.

It should be emphasized that the confidence estimates discussed here concern generalizations you could make regarding all potential clients. Thus you are treating the ones studied (even if they constituted all of your clients for the last X years) as if they were a random sample of all potential clients. If you simply want to extrapolate to the clients who actually were in your program and finished at the same time as your follow-up sample, then you have greater certainty in the accuracy of your estimates since your sample probably includes a sizable proportion--perhaps all--of the matriculated clients. At the far extreme, if all matriculated clients constitute the obtained sample, there is no sampling error in generalizing only to matriculated clients. In other words, the observed results are 100 per cent accurate from a sampling point of view and no confidence intervals need be calculated.

You should remember that the tables which give you sample sizes assume you have outcome information on every case in those samples. In choosing the size of sample for your follow-up study, you will need to allow for loss of cases by death, refusal, and inability to locate. It is suggested you should work for at least an 80 per cent recovery rate. Therefore, the size of your initial sample for each group will need to be one-fourth again as large as the numbers in the table in Appendix C.

Obviously, the number of needed sample cases is an approximation based on your estimates of how many respondents you can actually find, and on how big a difference there is likely to be between your two groups in their "success" rates. However, Appendix C will help you see that if you have small samples, it will take a really big difference to show up as statistically significant.

COMPLETION RATES

Almost no study is successful in gathering data on every subject included in the sample. In controlled experiments some subjects will drop out of the experimental or control groups before the experiment is finished. In other kinds of studies some subjects will not be found and others will refuse to be interviewed. The completion rate of a study refers to the proportion of the subjects in the original sample for which data are available when the study is completed.

The lower the completion rate, the more the study will suffer, because those subjects who have been lost from the analysis are likely to be different from those included. This is a particularly serious problem in follow-up studies of drug abusers. Former clients who refuse to be interviewed or who are unavailable for interview may also be more likely to have returned to drug use or to have gotten into trouble with law enforcement agencies.

There is no rule which states how high a completion rate should be before the results of a study can be considered valid. The closer the rate is to 100, of course, the more confidence the research public will have in its results. For follow-up studies a figure of 80 per cent is probably a reasonable target for planning purposes, and this projected completion rate should be part of the study design. In other words, samples should be drawn with the expectation that data will be available for only that percentage of the sample. If data from more than that percentage are in fact available for analysis when the study is completed, that will be a happy plus. If data from less than that percentage are available (certainly if less than 80 per cent are available), then serious consideration should be given to extending the data gathering effort so that the response rate can be increased before data analysis begins.

Those interested in a more complete discussion of research design might like to consult the following: Goode and Hatt (1952, chaps. 7 & 8), Selltiz, Jahoda, Deutsch, and Cook (1960, chaps. 3 & 4), Cambell and Stanley (1963), or Kaplan (1964, chaps. 15-19).

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Chapter 4

MEASUREMENT CONTENT

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Suggested Measurement

This chapter discusses the factors or variables which you may want to measure, and suggests ways of measuring them. As you read through the discussion of the many variables which might be included in your interviews, you will want to refer back to Appendix E for specific questions which have been developed to measure each variable. While other questions might be developed to get at the same information, the suggested items will provide an excellent starting place in most instances.

Of course, what you want to measure--the content of the interview-will be largely determined by the purpose of the follow-up. As discussed earlier in this manual, most follow-up studies will normally want to make certain comparisons of interest among different groups of clients. For example, in order to determine the relative success or failure of various treatments, you may want to compare current drug use, employment patterns, and criminal behavior for groups which experienced different types of treatment. In such comparisons the independent variable is the type of treatment, and the dependent variable is the current behavior as determined in the follow-up interview. Independent variables are simply ways of grouping the client population into meaningful categories so that the effects of treatment on the various client groups can readily be measured. Dependent variables are the measures of behavior such as current drug use, which may differ among the independent variable groups.

If differences in current behavior are found among the various treatment groups in the follow-up study, then the question immediately arises whether the differences are due to the different treatments, or whether they are simply a continuation of differences which existed prior to the treatment. If the necessary data have been gathered, you will want to compare the current behavior for each group with the behavior for each group in the time period prior to treatment. These pretreatment measures are generally termed baseline data. In addition, you may want to collect data during treatment. This would allow an assessment of the effects of treatment while the client is attending the clinic, as well as after he leaves it.

Whenever you compare behavior during two time periods, it is clear that the means of describing the behavior at both times should be as nearly identical as possible. It should be apparent that the data available for the first period (the baseline data) will set limitations on the content and form of the interview used to describe the second period (follow-up data). It may prove useful to explore these limitations in some detail.

BASELINE DATA

It is possible to draw a distinction between planned and unplanned baseline data. When researchers refer to planned baseline data, they mean that the data collection at entry into treatment is collected with the intent of carrying out a later follow-up. A very important advantage of such a prospective study is the opportunity to collect information which will aid in locating the client later for a follow-up interview. Since it is usually not known when the last clinic visit of a given client may be, this information should be obtained at the first visit to the clinic. Some important data to collect (for which sample questions have not been provided in Appendix E) are as follows:

- name
- aliases

- date of birth
- phone number
- mother's maiden name
- father's name
- names, addresses, and phone numbers for spouse, siblings, friends, and relatives--especially those likely always to know the client's whereabouts
- driver's license number
- social security number
- other numbers such as military service, Veteran's Administration, alien registration, or state criminal number

It is frequently the case that follow-up results are wanted quickly, without waiting for clients to enter treatment, complete it, and spend some time in the community thereafter. Therefore subjects are chosen whose baseline data were collected some time ago, before anyone had a follow-up study in mind. For federally funded programs, the available baseline measures are sometimes limited to the items contained in the CODAP collection forms. (CODAP refers to Client Oriented Data Acquisition Process. This is the federally sponsored system which collects standardized information on clients as they enter and leave drug treatment programs funded by the National Institute on Drug Abuse and other federal agencies throughout the country.)

The difficulty of locating clients who have not presented sufficient information at admission is an obvious problem. However, once the client is located, the necessary baseline data may be obtained by retrospective self-report at the time of the follow-up interview. While there may be errors of recall, this is one way to supplement a very limited set of baseline data obtained at the time of admission.

Fixed Versus Variable Data Reporting Periods

Many follow-up studies collect data on the dependent variables for a baseline period of one or two months prior to treatment entry and for a similar period immediately preceding the follow-up interview. An alternate approach is to collect these data continuously for as long a period as desired--for example, from the period of first narcotic use to the time of the interview. This method encompasses longer time intervals and often breaks them down into discrete periods which have particular meaning in a person's life, such as the time when a respondent was in jail, in treatment, addicted, or employed. This continuous recording is considerably more difficult than is comparing two discrete data collection periods of equal length. However, it provides several advantages over the simpler procedure of taking a fixed recent interval, such as the previous two months. First, baseline data collected just prior to treatment admission may be misleading.

Generally, the individual asked for treatment because he was using drugs more heavily and experiencing more legal or other difficulty than usual for him. Second, limiting the follow-up data period to the time just prior to the interview tends to produce less candid responses to sensitive areas. Respondents are typically more willing to admit past as opposed to current criminal activity and other socially disapproved behavior. Finally, brief baseline and follow-up data periods may not adequately sample relatively rare events such as overdoses or arrests and convictions.

Nevertheless, there are some important disadvantages to using variable time periods over which behaviors are reported. For the purposes of most readers using this manual, these disadvantages probably outweigh the advantages. For one thing, the question sequences are more difficult for the interviewers to administer and more burdensome for the respondent to answer. They can also take a great deal more interview time. In addition, proper analysis of the results from such questions requires more sophistication than is true for fixed-interval questions. Finally, for the purpose of comparing groups, rather than studying individual histories in depth, fixed interval questions provide reasonably adequate data. Therefore, in the sample questions which will be provided concerning behaviors like drug use, criminality, and employment, fixed intervals are suggested.

However, if you elect to collect follow-up data over a long period of time, as opposed to the one or two months preceding the interview, the following suggestions may be of help. First you will need to define how the continuous period is to be divided into intervals. For instance, you might wish to divide the client's history into the periods when he was (1) incarcerated, (2) in treatment and (3) not in treatment. This would usually define a number of specific time periods for which you would collect data on a limited number of variables such as drug use, employment and criminal activity.

Obviously, a special set of questions will need to be asked in order to determine the sequence of such important events in the client's life. Then, you can describe the period in terms that are meaningful to the respondent. For example: "Now I want to talk about the six-month period between the time you were discharged from the South Bay methadone program in June, 1972 and when you went to jail in December, 1972. You were 23 at that time. What was your narcotic use during this period? Did you work any, etc?" The interviewer should begin at the desired time in the respondent's history, and proceed consecutively with each interval to the time of the interview. Experience shows that better results are obtained if all the questions about a given interval of time are asked at once. If possible, the interviewer should avoid taking the respondent separately through drug use history, then employment history and so forth.

INTERVIEW DESIGN AND CONTENT

For the purposes of this discussion, it will be assumed that the follow-up data are to be collected via personal interviews rather than by mail or telephone. The potential number of dependent variables is fairly limited and will tend to be similar from one follow-up study to the next. On the other hand, there are many independent variables and the choice of which to include will depend on the purpose of the follow-up. It is important to define the purpose quite clearly by listing the major questions you want your report to answer. The questions must be quite specific. It is not sufficient to ask: "Is the treatment program effective?" Rather, you should ask: "Do participants who complete the program use opiates less frequently after the program than dropouts?"

You should examine each potential interview item for its contribution to these goals. It is tempting to include a wide range of items without a clear expectation of how they will relate to the purpose, but this invariably leads to unsatisfactory results. The amount of time the respondent is available for interview, and the cost and time available for the data collection and analysis, are limited. Therefore, you should focus on the primary questions to be answered before including less crucial items. You can often improve the quality of the results by examining a crucial question such as drug use from more than one perspective, or for a longer period of time. This approach is generally preferable to a more superficial coverage of a wide area.

Some Guiding Principles

Here are some principles to keep in mind in designing your interview.

Initially, in selecting questions to measure the dependent variables, it is desirable to emphasize those items which provide objective descriptions of behavior--preferably in quantitative terms for which well verified and standard measures exist. The reason for this emphasis is not that less quantifiable variables are unimportant, but rather that the methods for assessing them tend to be less precise and sometimes less reliable.

Second, it is preferable in general to design your interview with relatively neutral and nonthreatening questions at the beginning. This will permit time for the interviewer to establish rapport before asking items of a more sensitive nature. But these beginning questions should not be totally uninteresting, because you do not want to bore your respondent. One possible introductory question is: "How are things going for you these days--would you say very well, pretty well, or not too well?"

A third principle of interview design is as follows. When a question is asked which requires a choice among four or more alternatives, the use of a card listing the alternatives is generally recommended. This "show-card" can be handed to the respondent, who can then read the alternatives. (See Appendix E for examples.)

Fourth, for continuous variables such as age or amount of income, the interviewer should record the response as a specific number rather than as falling within an interval. This will enable the calculation of the mean, median, and other statistics from the original as opposed to the grouped data.

A fifth principle is that response alternatives should sometimes include a "Don't Know" category. However, the number of times this category is used should be minimized by careful consideration of the various available specific responses.

Finally, keep your questions as simple as possible. At first glance, size of family is a simple concept. You might want to ask: how many brothers and sisters do you have? But even this apparently simple question is difficult. What about step-brothers or -sisters? What about foster children who lived in the respondent's home? Should the respondent count siblings or step-siblings who never lived in the same house with the respondent? Do you want him to count or not to count siblings who died? The point is not that you must allow for every possible contingency in your questions; this would complicate life too much for most of your respondents. But you should be aware that even questions that appear straightforward may get very complicated.

MAJOR VARIABLES TO CONSIDER

The following three sub-sections list some of the more common variables appearing in drug treatment follow-up studies. Appendix E gives examples of questions used to measure each variable. In many cases, a variable will have been measured in the baseline data collection, and will not need to be re-measured. Such variables are marked with an asterisk in Appendix E.

While the items are listed under the three categories--independent, dependent, and other variables--it should be emphasized that this grouping is by no means absolute. A given variable is generally an independent variable when it occurred prior to entering treatment (type of treatment is an exception), and is generally a dependent variable when it occurs between entering treatment and follow-up. For instance, employment is an independent variable if you ask how individuals who had ever held the same job for two or more years before treatment differ in posttreatment drug use from those who have not held a job that long. But employment is a dependent variable if you ask whether persons who complete treatment are more often employed at follow-up than those who dropped out.

INDEPENDENT VARIABLES

1. Demographic data.

   - date of birth
   - sex
   - ethnic background
   - place of birth
   - education before treatment
     - highest grade completed
     - highest degree
     - age leaving school

2. Family background.

   - parents' education
   - parents' occupation*
   - religious background
   - broken home before 12
   - parents' history of alcoholism
   - parents' history of arrest
   - parents' history of opiate use
   - number of younger siblings
   - number of older siblings
   - siblings' history of alcoholism
   - siblings' history of arrest
   - siblings' history of opiate use

3. Military experience before treatment.

   - length of service
   - whether served in Vietnam

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*In the sample interview, father's (and mother's) occupation is recorded verbatim, then immediately coded by the interviewer. This coded value provides a kind of status ranking, from "1", a high status job, to "7", a low status job. (Codes 8 and 9 do not indicate any particular status ranking.) For analysis purposes, this coded value is very useful and because the interviewer codes the occupation immediately, any ambiguity or uncertainty as to the proper code may be resolved by getting more information.

4. Treatment received at this agency.

- date of admission this time
- date of discharge (if not still in program)
- continuity of treatment
- length of time in treatment
- number of entries into treatment before this admission
- medications received
- other services received
- reason for termination of treatment

One reason for asking these questions is to determine whether there are important differences in outcomes for different groups. For example, it may be the case that high school dropouts are benefiting most from program participation. Another reason for asking these questions is to allow selection of subgroups comparable to those in other programs. For example, a methadone maintenance program in New York may report that a certain proportion of their younger black clients are drug free at two years, and you would like to know whether similar young people in your program do as well. Still another reason is to allow comparison of the respondents in the follow-up study with those who could not be located for the follow-up interview. This comparison may help determine whether there are certain types of clients systematically excluded from the follow-up.

It is by no means necessary or advisable that all these independent variables be measured in every follow-up study. Care should be taken not to become overly ambitious. You should restrict your study to those questions that can be of value to you. Some variables you will definitely want because of their likely relationship with certain outcomes. Age and sex, for example, are related to employment and criminal behavior, so you will want to include them.

It should also be pointed out that many of these independent variables will not change between the baseline and follow-up data collections. They therefore can be ascertained equally well at baseline or at follow-up. However, the respondents' motivation to respond accurately may be very different when interviewed by program staff as they enter the program than it is later when they have left the program. Entry may have occurred under some degree of coercion, and this fact could color some of the responses. Thus for some questions, you may feel that it is useful to ask the same questions at both baseline and follow-up.

DEPENDENT VARIABLES

Employment

The following items concern employment (or other productive activities, such as schooling) in the eight weeks prior to the interview:

- occupational history
- number of days worked full-time
- number of days worked part-time
- type of work
- date started
- number of different employers
- amount of take-home pay
- major sources of support
- any schooling or training

Tobacco and Alcohol Use

Tobacco use may be readily measured in terms of the number of packs or fractions of packs of cigarettes smoked per day. Measuring alcohol is somewhat difficult because it is consumed in different forms, and the dimensions of abuse include both average consumption per unit of time, and the frequency of pronounced intoxication. Additional items which might be covered are:

- hospitalization/treatment for drinking problem
- arrest while drinking
- family/job problems after drinking

Drug Use

There are several ways of assessing drug use, depending on the needs of the study. If you simply wish to compare usage at the time of the interview with that for a prior baseline period, it may be sufficient to record whether the respondent has used each type drug within the previous eight weeks and the frequency of use. If, in addition, you wish to obtain data on whether he has ever used, the age at first use, age of first daily use (if any), and the year of last use, you can employ a grid such as that shown on the following page.

For drugs used daily, it may be useful to determine the number of times used per day and the amount and cost of the drug consumed. This is especially true for heroin, since the cost is often quite large and varies widely among daily users. In this connection, you may also wish to determine whether the cost of the respondent's drug use is totally or partially supported by drug dealing.

Criminal Activity

This is a difficult behavior to measure adequately. Data on arrests, convictions, and sentences represent only a very small proportion of the overall criminal activity. However, for some purposes they may constitute useful information for analysis, particularly since

TABLE 2

Example of Drug Use Data Table

Now I would like you to summarize your non-medical drug use history. For each drug group I'm going to read to you, please tell me: (1) if you ever used it; (2) the age of first use; (3) age of first regular use (daily or almost daily for a month or more); (4) year of last use; and (5) usage during the past two months. (SHOW CARD) This does not include medical use. (REPEAT EACH QUESTION FOR EACH ITEM AS NEEDED).

	(1)		(2)	(3)	(4)	(5)
DRUG GROUP	EVER USED		AGE AT FIRST USE	AGE AT FIRST REGULAR USE (daily for 30 days or more)	YEAR OF LAST USE	USAGE DURING LAST 2 MONTHS* (SHOW CARD)
	YES	NO				
A. Glue, spray cans, gasoline, etc.	1	2				
B. Marihuana or hashish	1	2				
C. Hallucinogens (LSD, mescaline, peyote, PCP, etc.)	1	2				
D. Oral amphetamines & other uppers (whites, benzedrine, dexedrine, etc.)	1	2				
E. Injected methamphetamine (meth, speed, etc.)	1	2				
F. Barbiturates and other downers (reds-seconal, yellow jackets, doriden, quaalude, etc.)	1	2				
G. Heroin	1	2				
H. Methadone or dolophine (On the street)	1	2				

* For last two months code:

1	not at all
2	less than once a week
3	1-4 days per week
4	5-7 days per week

TABLE 2 (Continued)

DRUG GROUP	(1) EVER USED YES NO	(2) AGE AT FIRST USE	(3) AGE AT FIRST REGULAR USE (daily for 30 days or more)	(4) YEAR OF LAST USE	(5) USAGE DURING LAST 2 MONTHS* (SHOW CARD)
I. Other opiates (opium, morphine codeine, paregoric, dilaudid, demerol, percodan)	1 2				
J. Cocaine	1 2				
K. Tranquilizers (valium, librium, miltown, etc.)	1 2				

*For last two months code:

1 not at all
2 less than once a week
3 1-4 days per week
4 5-7 days per week

respondents are likely to be more truthful about crimes for which they have been apprehended than about crimes for which they have not. If you choose to use arrests, convictions, and sentences as a measure of criminal activity, more reliable results may be obtained by recording these events over a fairly long period of time. The sample questions in Appendix E refer to a time period of only two months. But if your particular follow-up includes only individuals who have been out of treatment for a year or more, the sample questions could be revised to refer to a period of a year. In addition to arrests, you should also record periods of probation, parole, or other legal supervisory status as well as incarceration. Data on arrests and convictions will be more meaningful if organized by type of offense (crimes against persons, crimes against property, drug offenses, and so on), as done in the sample interview.

Some sample questions are also provided with which you might try to secure self-reports of criminal behavior for which the respondents were not apprehended. Specifics are not requested, though they might be useful to have, since they would be likely to increase the honesty in answering.

Client's Evaluation of Treatment Program

The follow-up interview allows an opportunity to determine the client's impression of the treatment program and the program's impact subsequent to discharge. This information can provide you with valuable information as to the strengths and weaknesses of the program from the client's perspective, as well as suggestions for improvement. You may want to spend a considerable proportion of interview time in obtaining information of this kind, since it will almost certainly be helpful to you. Possible items include:

- ease of getting to program
- treatment staff job performance
- gain of understanding of drug problems
- staff help with other problems
- staff concern for client as an individual
- how much individual treatment
- desire for more individual attention
- satisfaction with treatment
- program good enough to recommend
- program reputation among drug users
- things liked about program
- things disliked about program

There may be a number of other aspects of your program about which you would like to have your clients' impressions. If so, you should draft your own questions--perhaps following the formats used for the sample items listed above--and pretest them on a few respondents to see how well they work.

OTHER VARIABLES

1. Type of residence.
2. Living arrangement.
3. Marital status and adjustment.
4. Number of times married.
5. Number of dependents.
6. Urine sample. (This will be discussed further under validating measures.)

PRETESTING THE INTERVIEW

It is difficult to overemphasize the importance of adequately pretesting your interview. Pretests just prior to going into the field are essential because they can tell you how respondents will interpret the questions you ask, how smoothly the interview flows, and how long it takes.

Pretesting is also a useful aid in developing the interview in the earlier stages. Rather than trying to anticipate all the questions and how to ask them, it is usually more efficient to begin with a rough draft, administer it to two or three persons, revise, administer again, and revise again, until the interview schedule appears to be set. In revising, you should go over each question with the interviewer to confirm that your intentions are being carried out.

You should also be timing the various segments of the interview during the pretest. Include a place (and instruction) for the interviewer to record the time at various spots in the interview. Then you can see how long the interview is running, and what segments may need to be shortened.

A structured interview is one in which all questions are asked in a predetermined order, with a precise wording for each question. This is a highly desirable feature for an interview, since it reduces the possibilities for interviewer bias. Questions may be open-ended, where verbatim responses are recorded, or closed-ended, where the respondent chooses from a set of alternatives provided to him. Closed-ended questions are preferable wherever possible, since they usually facilitate the task for both interviewer and respondent, and they are ready for data processing as soon as the interviewer circles a code.

The early drafts used in pretests can be relatively unstructured, with many open-ended questions. However, as you approach the final interview, you should firm up the content until you have a very structured interview, with as many closed-ended questions as possible.

If at all possible, you should have a professional survey researcher check your final interview for clarity of questions and responses. A person skilled in constructing interviews can do this in a short time and will normally provide numerous suggestions for improvement.

If you plan to make use of automated data handling, you may want to precode the interview responses and list the data card column numbers directly on the questionnaire.

In doing this, you should remember that when more than one answer to a single question is allowed, each possible answer must be assigned its own column, in which its presence or absence is coded.

VALIDITY CHECKS ON IMPORTANT MEASURES

You may want to check the accuracy of the self-report data by obtaining independent measures of certain items in the interview.

Employment

For employment, the Social Security Administration will provide grouped data for research purposes, showing the percentage of a specified list of individuals for which earnings were reported during a specified time. Such data are not available on individuals and you must provide the social security numbers. No information is included on employment for which social security payments were not made. In addition to providing a validity check on the self-reported information, social security data also provide another means of determining whether reported employment for the overall sample changes following treatment, or whether reported employment differs for subgroups within the sample.

Criminality

If criminal records (rap sheets) are accessible for your sample they can be compared with self-report data on arrests, convictions, sentences and legal supervisory status. (See Chapter 6.) Sometimes these records are incomplete, especially with regard to short jail sentences and periods of probation. You should not expect to find perfect agreement between the official record and the subject's report of the particular charge, since the charge often is changed in the course of coming to trial. In the event you are employing continuous data collection periods, prior information on periods of incarceration and legal supervisory status will enable you to better structure the discrete intervals about which the respondent will be questioned.

Drug Use

The use of urinalysis to validate self-reported data on current drug use is controversial. Those opposing this procedure have argued that it is not conducive to the establishment of good rapport between the interviewer and respondent; that those respondents not correctly reporting current drug use will not provide a urine specimen for the same reason; that, at best, the urinalysis only gives information on current use; and that the poor validity of urinalysis results obtained through commercial laboratories severely limits their usefulness for checking self-report data. This last point is an important one; many people believe incorrectly that laboratory analysis of urine sample provides a highly accurate and valid determination of what drugs have been used. In fact, errors are very often made. An analysis may incorrectly identify which drug is present, may fail to detect the presence of drugs, or may incorrectly report a drug to be present when it is not. These errors may occur because of machine inaccuracies, human errors in testing, or improper record keeping. For these reasons, it is often advisable to include in the samples sent for laboratory analysis some samples whose drug content is known. While not foolproof, this method may lead to greater or less confidence in the laboratory reports.

All of the above arguments against urinalysis have merit. However, current drug use falls in the category of sensitive responses and there is good reason to expect under-reporting even under optimal conditions of rapport. As mentioned earlier, with the proper interview procedures and interviewer skill, you can expect to obtain accurate answers within the limits of recall ability for those areas where the respondent has nothing to lose. Current illegal behavior does not fall in this category and an unknown proportion of your sample will likely deny or minimize their current activity in these areas. The strongest argument in favor of collecting urine specimens is that it enables you to establish some confidence limits concerning the self-report data on current drug use. Various follow-up studies have demonstrated that it is feasible to obtain urine specimens from ninety per cent or more of the respondents interviewed. Given this high a response rate and reasonably accurate laboratory analysis, you will be able to conclude that the rate of current drug use is probably somewhere between the number obtained by self-report and the sum of the following:

- self-reported use
- denied use, but had positive urine
- refused to provide specimen

One procedure that has resulted in a high rate of cooperation with respect to obtaining urine specimens is to offer an added monetary inducement at the end of the interview for this additional cooperation.

DEVELOPING OTHER MEASURES

Again, the specific questions provided in Appendix E are meant to give you some fairly well developed alternatives from which to choose. There may be other variables which will better meet your needs. Similarly, it may make sense to revise some of the questions provided--for example, by changing the time interval about which the question asks. The point is that there is nothing sacred about these particular measures, and you should feel free to develop your own to whatever extent makes sense to you. However, you should pretest such measures carefully before actually adopting them in your study.

Chapter 5

ETHICAL CONSIDERATIONS

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Federal Regulations

The protection of an individual's privacy has long been considered an important social value. Both the federal and state governments have rules and regulations protecting an individual's privacy. The discussion in this chapter covers a few of the more important federal laws and regulations in this area.

Regulations for the "Confidentiality of Alcohol and Drug Abuse Patient Records" have been issued by The Department of Health, Education, and Welfare (H.E.W.). They constitute Part 2 of Title 42 of the Code of Federal Regulations, and were originally published July 1, 1975, at pages 27801 through 27821 of Volume 40 of the Federal Register.

These regulations attempt to strike a balance between the need to do research in drug and alcohol problems, and the preservation of the privacy and anonymity of those who receive services. As outlined in the regulations, all treatment records are confidential and may not be used in any civil, criminal, administrative, or legislative proceeding without the client's written consent. However, consent is not required for disclosure of such records for the purposes of research, audit and program follow-up.

Interpretation of these regulations depends on whether the program research is voluntary or compelled. For programs which voluntarily participate in research projects, the interpretation of the regulation is left up to the researchers and the program. On the other hand, a program may on occasion be compelled to participate in research--for example, by a condition of funding or licensing of a governmental agency. To cover such situations detailed procedures are set forth in the regulations. In general, anyone obtaining such information is prohibited from disclosing it in any manner that could identify the clients. These regulations, then, serve a double purpose: to make the work of medical and social service institutions more effective, while at the same time making treatment acceptable to individuals who may have hesitated to seek such services in the past for fear of repercussions from employers or friends.

RISKS TO THE CLIENT

Ethical issues arise at every stage of a follow-up study--when locating the ex-client, when interviewing, and when processing and analyzing the data. There are various ways in which a client or ex-client could be harmed by participating in a follow-up study unless proper precautions are taken: (1) if in the course of searching for him at follow-up his status as a former drug user were revealed to employers, relatives, or friends who did not previously know it; (2) if in the course of being located for interview, his current whereabouts or his identity as a drug user were revealed to creditors, police, or others whom he does not wish to have this information; (3) if information about illegal acts or discreditable behavior in his treatment record or in his follow-up interview should get into the hands of police, creditors, relatives, or friends; (4) if criticism of the treatment program expressed in interview, or violations of its rules while he was a client, should get back to the program without his consent; (5) if the interviewer should use the interview relationship as a means of treating the ex-client against his wishes or without proper supervision, or if the interviewer should exercise improper influence on an ex-client to re-enter treatment or change programs; (6) if in writing up results, sufficient details about individuals were given to permit their identification. Each of these problem areas will be discussed in turn.

The Risk of Relapse

There is one further risk that may cross your mind: will recalling his drug history during the follow-up interview reawaken the drug craving of a former client now off drugs, or create undue anxiety or distress when he recalls his addiction? While reviewing the past might be unpleasant for some ex-clients, most appear to enjoy reminiscing, and to welcome the interest shown in them by being included in a follow-up. There has been no report of relapses attributed to participation in such a study.

Although there is no evidence that follow-up interviews actually increase the risk of relapse, it is probably prudent not to devote much time in the follow-up interview to a detailed recounting of the "highs" previously enjoyed. It is also important to remind the ex-client that his participation is voluntary, that he may refuse to answer any question, and that he can always discontinue the interview if he wishes. With these options open to him, there seems little danger that the interview will cause undue distress even to the occasional sensitive ex-client.

PROTECTING THE CLIENT DURING THE SEARCH PERIOD

Assuming that the experience of being interviewed does not in itself pose a serious risk, special precautions are necessary to protect the client against the risk of improper disclosure of information, mentioned above.

As suggested in Chapter 6 of this manual, the danger of revealing the ex-client's identity as a drug user while trying to contact him for an interview can be minimized in two ways: (1) arranging ahead of time with the client about people who can be contacted for an address and how they will be contacted, and (2) having the follow-up carried out by a group with an institutional affiliation that does not itself give away the fact that he was a drug client.

Informing the Patient at Intake

If possible, the client should be told at entry into treatment that he may be contacted for follow-up, and asked for names, birth dates, addresses and telephone numbers of persons who will always know his address. He should then be told precisely how the inquiry for an address will be made. For example: "I am Mr. __________ from the _____X_____ Research Group. We are trying to reach Mr. __________ because he was seen in a health program some years ago. We are contacting people who received services from various (state, county, private) clinics to see how they have gotten along since."

Letters to Clients

Letters to ex-clients and their relatives should be sent in envelopes with letterheads corresponding to that of the "____X____ Research Group." These should be equally noncommittal in content, because it is not at all certain that the ex-client will open his own mail, or if he does, remember to put it away after reading it. If possible, you should prepare the letter ahead of time and show it to the client at intake so that he will recognize it later - and not throw it in the wastepaper basket.

Neutral Auspices for the Study

These precautions against revealing the client's identity as a drug user require that if the follow-up is to be done by members of your own program, they should identify themselves in the name of their parent organization, such as the Veterans Administration, the state department of health, or the university. Permission to use the parent organization's name will ideally be arranged ahead of time, so that the client can be told at intake. If the parent organization is not willing to be listed as the sponsor, other alternatives include sponsorship by an outside organization, creating a research group with an innocuous name, or communicating under a physician's name, with an office address but no institutional affiliation listed.

PROTECTING THE CLIENT'S IDENTITY FROM CREDITORS AND THE LAW ENFORCEMENT SYSTEM

Protecting the Client When Searching Agency Records

In locating a former client it will sometimes be necessary to search police records, credit bureau records, or records of other agencies. It is ideal to do your own search, so that you do not have to turn over a list of names, and so that you can be sure the search is done thoroughly. However, if the agency wants to do its own searching, it is necessary that the list of clients submitted does not enable that agency to identify the persons on it as drug users. One way to protect the list of names is to salt it liberally with names of invented persons with age and sex similar to the client group. You can then tell the police or other cooperating agency the true purpose of the study, but also assure them that only some of the persons being sought are ex-clients.

Never Swap Information

Some agencies you contact in seeking clues to a current address may be looking for the same person you are. They may offer you some old information but in turn want you to tell them if you find something more recent. In such a situation you should make it a rule never to swap: you can receive information, but you must never give any. This even pertains to persons who, so far as you can tell, mean no harm to the ex-client: if his own mother does not have his address, this may be because that is the way he wants it. In such cases you should do no more than offer to tell him she wants him to get in touch with her if you should find him.

PROTECTING THE CLIENT DURING THE INTERVIEW PERIOD

Data in Agency Records

In ordinary opinion surveying, it is very easy to protect the confidentiality of the interview just by excluding identifying data from the record. This is not so easy in follow-up studies because in most cases you want to be able to link that interview to information in the client's record in order to learn what kind of pretreatment history and treatment experience leads to success or failure. As a result, special procedures to assure confidentiality are advisable.

The first step is to decide what it is out of the client's earlier record that you may want to relate to his outcome, and to transfer that information to another record which does not identify the client by name, social security number, birth date, address, or hospital number. To achieve this purpose the information may be transferred in coded form, thus getting rid of irrelevant detail such as relatives' names or precise date of entry into treatment--data that might identify him even if all other identifying data were removed. The coded abstract of his pre-treatment and treatment information should then be given an arbitrary identification number--not his hospital or clinic number. The list linking his identity with that arbitrary number should then be physically separated from the record, and kept locked up.

Data Obtained in Interview

As soon as the follow-up information has been collected, all identifying data should be removed from the interview and replaced by the client's arbitrary identification number. His follow-up information can then be linked to his coded treatment record by means of that arbitrary number. But if this procedure is followed, neither can be identified with any individual without the locked-up list that pairs that number with a name or a hospital number.

With this system, there is not even any record that can breach confidentiality. However, coders of treatment records and the interviewers will still know the client by name. They must be reminded frequently that no names are to be breathed outside the research group. This means not even in the hallways and elevators of the building, much less outside.

Obtaining Immunity from Subpoena

While these precautions protect interview data against careless disclosure, what about the dangers of subpoena? After all, if both the locked up list and the interviews were subpoenaed, individuals could be identified. Several federal laws provide protection against subpoena. For example, section 408 of Public Law 92-255 protects drug clients' records and drug research against unauthorized disclosure. Such records may only be subpoenaed by a court of competent jurisdiction as outlined in the regulations.

Requesting Immunity from the Justice Department

Further protection can be obtained under Section 872(c) of Title 21 of the United States Code, which vests authority in the Attorney General to give a grant of confidentiality to any drug research project. Section 0.100 of Title 28 of the Code of Federal Regulations, as amended July 10, 1973 under 38 F.R. 18380-82, delegates this authority to the Administrator of the Drug Enforcement Administration of the Department of Justice. This protects drug research records against all subpoena while the project is active. To qualify for immunity, your follow-up study must request the protection of the Department of Justice under this law before the study actually begins, so that there will be no uncovered period.

Requesting Immunity from HEW

A similar kind of immunity from subpoena can be obtained under Section 242a(a) of Title 42 of the United States Code, which vests authority in the Secretary of Health, Education, and Welfare to give a grant of confidentiality to "persons engaged in research on mental health, including research on the use and effect of alcohol and other psychoactive drugs." On December 4, 1975, proposed rules were published in the Federal Register in regard to the exercise of such authority (40 F.R. 56692), but at this writing (April 7, 1977) such rules have not yet been adopted. Despite the absence of regulations governing procedure, the Secretary has the authority to act under the statute, and has done so on a number of occasions. An application for a grant of immunity under this statute with respect to research related to drug abuse or treatment for drug abuse should be made through the Director of the National Institute on Drug Abuse. Under the practice followed by HEW, such a grant of immunity would be in perpetuity, not in the sense that the project can go on forever, but that records generated while the project is going on continue to be protected after it is terminated. Also, under HEW's practice, the grant might be subject to certain conditions designed to prevent fraud and to deal with medical emergencies, but these would not ordinarily appear to have any relevance to research or investigation which in itself contains no element of administration of drugs or delivery of services.

THE CLIENT'S CRITICISM OF THE DRUG PROGRAM

Confidentiality Within the Agency

One important goal of the follow-up study is learning the client's opinions about the drug program or his history of compliance with rules while in the program. But it is virtually impossible to keep these confidential without employing outsiders as interviewers. If this information is not kept confidential, there is a danger that the client's criticisms of the program, or his revelations of rule-breaking, will influence how he is welcomed back if he decides to return, or his future treatment if he is still in the program at follow-up.

Informing the Client

In large agencies, there may be a separate research unit which is able to do follow-ups and still preserve confidentiality within the agency. But in small-agency, small-budget studies, this will not be possible. If treatment personnel will be doing the interviewing, you should at least be sure the ex-client is aware that those who treated him in the past or who might treat him in the future may find out what he said about his treatment experience. While informing the client makes asking him about his opinions of his treatment and about his compliance with treatment acceptable from an ethical viewpoint, it may lead him to so distort his answers that they are not scientifically useful. Therefore, if at all possible, you should try to use interviewers who are not also part of the treatment staff.

IMPROPER INFLUENCES ON TREATMENT

Improper Advice By Interviewers

At the time of the follow-up interview, ex-clients may be in medical or legal difficulties. Special precautions are necessary during this interview period to avoid forcing the ex-client into undesired or improper treatment. It is tempting for a sympathetic interviewer, particularly one having some treatment skills, to try to help the ex-client. However, the ex-client has the right to seek the form of treatment he prefers, or not to seek any treatment. It is important to recognize that advice volunteered with respect to a drug abuse or emotional problem--both of which are often handled by psychotherapy or legal counsel--can easily be interpreted as treatment. In any case, it is not ethical for an interviewer to take a treatment or counselor role without an explicit request from the ex-client. If the interviewer is so requested, he may suggest that the ex-client reenter the program formally, so that no question can arise that the treatment is not properly supervised.

Ethical considerations and scientific validity argue against offering unrequested advice during the follow-up survey. If advice is requested, the interviewer can say, "We'll talk about that right after the interview is complete." In this way any advice given (or any refusal to give advice) will not bias the remainder of the interview. If the client is in trouble but has not asked for advice, the interviewer should only volunteer: "If you should ever want help with that problem, you could call ____X____ agency."

PROTECTING THE CLIENT WHEN PRESENTING RESULTS

No program would be so thoughtless as to publish actual names of subjects when reporting results from a study. However, it is often tempting to present brief case histories to illustrate various kinds of outcomes. It is well worth considering that even without names, these case histories can often be identified by persons who know a few of the background facts. For instance, a man accused or convicted of a crime may be well known locally; but it may not be known that he was a former drug addict. Describing his crime may be enough to identify him for people in his home town and thus expose him as a drug user.

Clearly, such vignettes describing individuals should be used sparingly and should be stripped of all the circumstantial detail that makes them identifiable. Unfortunately, this often makes them much less entertaining reading. While a report that sticks to statistical tables without case reports may be less readable, this will not affect its value as a guide to future planning. Moreover, such a presentation is much less likely to endanger the confidentiality of the subjects who made it possible.

CONDITIONS UNDER WHICH DISCLOSURE IS ETHICAL

The confidentiality considerations mentioned thus far assume that the ex-client does not want information he gave in the follow-up interview revealed. However, it sometimes happens that the client does want some information given out sometime later--usually either to his lawyer or to his doctor. Therefore, you should be careful even when someone appears with a signed permission slip from the ex-client. The ex-client may not know exactly what information you have about him or how it looks when written down.

In such cases the best procedure is first to turn such records over to the ex-client himself. He can then look over a transcript and decide whether all of it or only parts of it are relevant and helpful to reveal for the intended purpose. The fact that the client has signed a permission slip relieves you of the legal responsibility but not of the moral obligation. When he signed the slip, it probably never occurred to him that he would actually be able to screen the contents himself.

Chapter 6

LOCATING RESPONDENTS

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Why Drug Abusers Are Hard to Locate

A follow-up study of drug program clients presents all the difficulties found in any follow-up study, plus some additional problems. In particular, unique issues are involved in locating the subjects. As a rule, drug abusers will be difficult to locate because many are more mobile and less often in contact with their families than average follow-up subjects. These subjects also represent an extreme case for the following reasons: (1) most are young males, the most mobile group ever among non-deviant populations; (2) they are usually very poor, which further increases their mobility; for example, they may be evicted for not paying the rent, or may move to dodge creditors; (3) they are less often registered in many of the sources that readily provide changes of address for most people, such as the rosters of electric and gas companies, telephone books, voting records, drivers' licenses, or alumni records; (4) they often have poor ability to sustain interpersonal relationships, which means that their earlier spouses or mates may no longer know where they are, and that relatives may not have heard from them for a long time. Further, former drug abusers may be difficult to find at home even after home addresses are located, because those who have relapsed will be out "hustling" and "copping."

Significance of Treatment Success or Failure

These reasons for unusual difficulties in locating drug treatment clients for follow-up have been listed as though all former drug clients were alike. Of course, they are not. Some are a great deal easier to locate than others. Those who have remained in the treatment program longer will be easier to locate because their habits and interests will be better known and because their longer records will provide more clues for locating them. In addition, their latest address of record will be more recent. Further, those who did well, whether as a result of treatment or spontaneously, are more likely to be living a conventional life, with all that implies about less moving from one address to another, more contact with wife and relatives, more community participation, and consequent ease of location. At the same time, those who did not do well may be back in treatment, or in a prison, and may also be found easily. Since ease of location is almost certain to be associated with extremes of outcome, recovery of nearly all the cases is necessary to avoid having a biased estimate of program effectiveness.

Treatment Failures

Although in general clients with poor treatment results will be more difficult to locate, the most extreme treatment failures--persons who are dead or in prison--are easy to document. Records showing their failure are found fairly easily. For this purpose the vital statistics divisions in the state departments of health can be very helpful. By submitting names and dates of birth, it is possible to verify death certificates or obtain birth records, which in turn supply names of parents and places of birth. Such information also helps the interviewer determine whether or not he has found the correct subject within the death or prison records.

This problem of locating respondents is important because a follow-up of clients which does not locate almost all subjects within the chosen samples will produce results that are biased both positively and negatively. Those most often missing may be the less extreme failures: namely, those clients who returned to addiction without coming to further public notice.

COLLECTING DATA PRIOR TO DISCHARGE

Intake Data

At the outset, it is necessary to ask what kind of data may be needed to locate clients at some later time. Planning for data collection can greatly reduce the difficulties of later follow-up. To assist in the locating effort, it is extremely helpful to collect some basic information routinely when the client first applies for treatment. Such information might include items like the following: (1) address and telephone number; (2) social security number; (3) occupations held; (4) aliases used; (5) date and place of birth; (6) parents' names and birthdates; (7) siblings' names and birthdates; (8) name and birthdate of wife or girlfriend; and (9) names and birthdates of children. Having such data as these in the file will not only help later in using agency files in a locating effort, but will also help the interviewer determine whether or not the correct person has been found before proceeding with the interview.

Planning for Later Access to Records

A few years ago, a collection of information of this type would have been adequate to assure locating almost every sample member even after years had passed. The names and birthdates could be checked through police and school records, and through voter registration and drivers' license registration, until a current address of a relative or friend on the list was obtained. That person could then be asked for the subject's current address. However, for various reasons, many of these agency records are no longer open to the researcher without written permission from the subject.

Getting the Client's Consent

Thus if possible the client at intake should provide the researcher with permission to look at all the pertinent records. It is helpful at the outset, then, to ask the client to sign a permission slip to look at vital statistics, credit ratings, police records, hospital and clinic records, driving records, welfare records, unemployment compensation records, and school records of his children if they are old enough. It should be emphasized that the permission slip should be obtained on the very first appearance of the client in the program office. Otherwise, dropouts will be harder to follow than those who continue in the program. This means that the client will need to be told immediately that there is an interest in following him later, and will need to be assured that later attempts to locate him will be extremely discreet. (See Chapter 5 for suggestions about how to do this.)

Informed Consent or Coercion

Obtaining such a signed consent form presents both psychological and ethical problems. First, requesting the permission described above may cause the client to feel he is being pressured to cooperate. Your attempt to obtain informed consent may imply to him that he must do what you ask in order to be admitted into the program. It is important to make clear that his receiving service is not in any way contingent upon his participating in any follow-up.

Ethical Risks

Second, even though you may be successful in gaining proper access to other agencies' records, certain ethical risks and considerations will inevitably arise. Again, matters such as these are discussed more fully in Chapter 5, and it is essential that you follow the procedures explained there.

COLLECTING DATA AFTER DISCHARGE

If you have not foreseen that you would want to do a follow-up on your subjects, and now decide to do so only after the individuals have already left your program, your task will be more difficult, but not impossible. Systems used to locate hard-to-find persons such as drug abusers may be conveniently grouped into four somewhat overlapping categories: (1) public records and friends; (2) the criminal justice system; (3) the "helping services" system; and (4) the sub-culture of addiction. Each of these will be discussed in turn. Wherever possible, these efforts should be centralized.

In launching a search through these records, you should use a "search sheet" for each person you are trying to locate. Each new item of information found--such as correction of birth-date, a middle name previously unknown, an alias, or a relative's name or address--should be entered on this "search sheet" to be used when making inquiries at the next place. The more complete the identifying information you can give, the better the chance of finding the records that you seek, and of recognizing that a record with incomplete identifying information does indeed belong to your "lost" subject.

Public Records and Friends

Public records are usually the first source to be employed because they are so readily available. To begin, you might use city directories and the telephone company's directory listings, plus the help of information operators to try to find the subjects--or, if they are not listed, members of their families or perhaps girlfriends named in their records. If this fails, you might try the street address directory which may be rented from the telephone company. Because this directory lists numbers by residence instead of name, it allows you to call former neighbors who might have information on where your client has gone.

Another major source of help is the United States Post Office. Not only may letters be sent to subjects either directly or in care of someone else, but also the postal system's records of address changes are very useful. For a small fee, the post office will check and correct up to 20 names and addresses if there is an annual change-of-address notice on file. The "special delivery/return-receipt-requested" service is especially helpful, because you learn the name of the person who accepted the letter. Moreover, a special delivery letter makes the study seem important to the subject, so that it is more likely that he will read the letter carefully. Finally, it may be helpful to talk to the local postman assigned to the area in which he is thought to live now or from which he moved.

After leads emerge from one of the above sources, locators can make home visits and talk with friends, neighbors, and occasionally landlords for personal leads. The local bar may also prove to be a good resource. In addition, the state motor vehicle bureaus may be quite helpful, since most young men have driver's licenses and the application requires birth date and current address.

The Criminal Justice System

Many if not most drug abusers, especially narcotic addicts, eventually become known to the various local, state, and federal law enforcement agencies. These agencies can be contacted if routine methods of finding a subject have proved unsuccessful. Issues of confidentiality are of great concern here, and all the recommendations about privacy discussed in Chapter 5 should be considered carefully, particularly those about not divulging information beyond that already known about your study population.

Police and Prison Records

Local police arrest records generally include dates of birth, dates of arrest, and addresses. They may also include calls for ambulances and reports of thefts. These always include addresses, but may be hard to identify if the client's name is a common one, since they do not include birth dates. If any of your subjects has been convicted of a felony, the social histories in state and federal prison records may contain information that is helpful in locating these subjects. Particularly useful are the names and addresses of visitors during any period of incarceration.

Federal Records

In addition, the Federal Bureau of Investigation accumulates records from all the states. Access to these records is valuable if some of your subjects have moved out of state during the follow-up period. Also, the cooperation of the Bureau of Prisons and the United States Parole Board can be very helpful. Before these agencies will work with researchers, they will require that their needs for the protection of data be met. Although many subjects may not currently be known to these federal agencies, their records may include information not available in your treatment record. Once a subject has been located in federal records, you can contact the last arresting agency and learn from them what the disposition of the case was. If the person has been placed on probation or incarcerated and then paroled, the state probation and parole office is very likely to have a current address.

The Helping Services System

In addition to the vital statistics divisions in the state departments of health, mentioned previously, rosters of other agencies which can be consulted in efforts to locate subjects include Medicaid, social and welfare agencies, mental hospitals, and Veterans Administration hospitals. These records are a useful resource in locating clients but they may have added value in that checking the complete list of subjects in the follow-up study against the current and past rosters of these agencies can be used as a way of measuring treatment success.

Selectivity is advisable when sending names to other states for checking against their records. However, many out-of-state agencies prove very cooperative in searching their records. When they identify an individual, they may also search further in base files to secure addresses and next-of-kin information.

Finally, visits to various drug abuse programs may help to locate clients who have moved to a different program since leaving yours. Remember that you must be careful not to divulge confidential information even when talking to another treatment program.

The Subculture of Addicts

In addition to the methods described thus far, you may also seek aid from subjects already interviewed. For example, after a subject has completed his interview, he might be asked whether he would like to help find some still unlocated subjects, and a small fee might be paid for successful leads. Frequently persons who attended the same treatment program know each other or have friends in common. The following technique has proved useful to researchers in the past. You might read the list of unlocated persons aloud, giving only the last name or only a nickname. If the helper recognizes a name, he is asked to supply a first name or an address or the area of the city frequented by the unlocated person. What he says is recorded. If the information provided by the helper matches the information already at hand, it may be assumed that you are both referring to the same person. But the helper should not be told which are "hits" and which are "misses." In this way, no confidence is betrayed to the helper, and the burden is on him to convince the investigators that he really knows the person sought. In this situation, too, it is important to make clear that everything said is protected by the laws of confidentiality as well as by your own guarantee of privacy and anonymity.

It should be explained to these helpers that you want to make contact with the unlocated subjects for an interview; therefore, they should respond only to the names of persons whom they know well enough to find directly. Experience shows that interviewed subjects are typically quite cooperative and willing to help if they can. The above technique can be extended by requesting information from other former addicts and offering them the same financial incentive.

CONFIDENTIALITY, PRIVACY, AND PROTECTION OF REPUTATION

The importance of confidentiality, privacy, and the protection of the reputation of study subjects and their families cannot be too greatly emphasized. These matters should be addressed at every stage of the research process--when locating subjects, when making personal contact with them, when gaining informed consent and conducting the interview, and finally when processing the interview data. Experienced researchers have found that it is possible to fulfill their responsibility to observe confidentiality, while still getting the data they need.

Withholding Identification

In order to fulfill this responsibility, two general operating principles should be followed throughout all the stages of the research. First, sample subjects should never be identified to any person or agency as narcotic users, unless this information is already known to be in their possession. Two corollaries of this are as follows: (1) never reveal the drug abuse nature of the research to any person or agency that knows the subjects but does not know about their drug use; and (2) never reveal the exact identity of a subject when the drug abuse nature of the research is known.

Withholding Information About Location

A second guiding principle is never to reveal to any person or agency the exact location of any sample subjects who have been located. Indeed, you should never conclude an inquiry at any agency or with any individual that leaves them in possession of more information about a subject than they already had when they were first contacted.

Conducting the Study Under Neutral Auspices

If possible, it is desirable to conduct the survey under the auspices of a broad umbrella agency, such as a health department or a social services organization. The intention is not to hide the true nature of the research project from the subjects, but rather to protect their reputations and privacy. Any possible risk of misleading subjects in this first contact is well worth the price of losing the one or two who might feel betrayed if someone else were to find out about their history of drug use.

The Health Emphasis

There are at least three workable approaches to the task of creating plausible yet non-threatening auspices for your study. One is to work through the office of the medical director if your agency has such a person. Emphasize that the study has medical backing and seeks data related to health. A second approach may be appropriate if your medical director or another physician within your agency has an active relationship with the local or state medical society (or both). In such a situation, investigate the possibility of the society's sponsorship together with the physician's personal involvement. A third approach is to speak generally about a "health survey" and to state that your study is part of an overall effort to acquire information about such factors as health needs or treatment facilities.

Neutrality of Initial Letter to Subjects

An initial letter is usually sent to all subjects explaining the study and expressing the staff's wish to interview them. This letter should be worded very carefully to interest the respondent and to avoid scaring him off. In particular, the letter should not contain any drug-related terms or any references to drug use, drug abuse, or research about drugs.

The following letter was actually sent to the subjects of a recent survey. Care was given to using stationery whose letterhead was not threatening: it simply identified a neutral grants-administration agency under whose umbrella the survey was conducted. The text of the letter simply states that the purpose of the project is to study changes that occur in people's lives, and that respondents have been selected at random. The name of the study director, the source of supporting funds, promise of payment for cooperation, and guarantee of confidentiality of information are all specified in the letter, which appears below:

Date

Dear __________,

You have been specially selected to be a participant in a research project that we are conducting. We are (name of agency), a non-profit research organization. This research project is under the direction of ___________, and concerns the changes that occur in a person's life and the effect they have on his well-being. Your participation in this study will involve giving us an interview, for which we are going to pay you.

We would appreciate it if you would give us a call as soon as you receive this letter. If you are out of town, call us collect. This will allow us to explain more fully what the interview involves as well as find a convenient time to interview you. There is usually someone in our field office every weekday between 8:30 A.M. and 4:30 P.M. If we do not hear from you after a reasonable length of time we will telephone or call on you to explain further what our research involves and why we want to talk with you. Our telephone number is ____________.

Sincerely yours,

____

Another example of a letter of introduction for interviewers appears in Appendix F.

DIRECTIONS FOR IN-HOUSE STAFF

Tasks Which Office Staff Can Handle

The office staff can take charge of handling telephone calls and searching agency records. As noted earlier, the initial letter you send to your sample subjects will presumably ask them to call your office to make an appointment, or to obtain answers to any questions they may have. The office staff should have a copy of this letter always at hand for their own reference, plus written instructions about the procedure for making appointments, and a list of questions that respondents are likely to ask. Answers to these questions need to be provided also. This information and advance preparation enables the office personnel to handle other telephone inquiries as well, especially those made by agencies and certain individuals whose assistance the study requests. It may be helpful to install a special telephone line for the duration of the study so that all calls related to the study can be received at one number. Then the staff will always be prepared to respond appropriately when the designated telephone rings.

Special Training for Office Staff

People who answer the phone must be prepared to answer the questions of respondents who will be calling in. Scripts prepared to guide secretaries in answering telephone calls from study subjects should be carefully designed. Particularly they should not contain any reference to drugs, drug use or abuse, or study of drug users: there is no guarantee that the person on the phone is really the research subject even though he may say he is. Secretaries and research assistants should be instructed never to bring up the subject, and not to mention any of the information the study already possesses about the individuals in the sample. Rather, they should be instructed to repeat only what the letter has already communicated. They should also be instructed to say that they are not at liberty to divulge confidential sources of information, a principle the subjects are sure to understand inasmuch as this same protection would be extended to any information given by interview subjects. Finally, in answering the telephone, secretaries should always identify the office with neutral words such as "Social Research Center." Not until a sample subject presents himself at the study office or meets an interviewer in an off-site interview should the complete nature of the research project be revealed. The purpose of these procedures is not to deceive the client, but rather to protect him from unintentional exposure.

Sample Instructions for Staff

Below are examples of prepared scripts that were actually used in a recent survey. Note that they contain no references to drugs, drug use or abuse, or study of drug users.

HOW WAS I PICKED?
WHY WAS I PICKED?

"Your name was selected from a list of many who could have been chosen, by a scientific procedure called "random sampling." What this means is that you will be used to represent a number of people who are much like you, and instead of interviewing all of those people, we have chosen you. Your selection was a matter of chance. Any person could have been selected, but your name was the one that happened to come up. This is the way all of the people who are a part of this study have been chosen. We want to interview everyone because if we should fail to interview someone who has been selected, not only will that person be missing from the study, but all of the other people like him will be left out as well. And that would not be good. For this reason it is important to get your cooperation and to set up a convenient time for your interview."

WHAT QUESTIONS ARE YOU GOING TO ASK?
WHAT DO YOU WANT TO KNOW?
WHAT IS THE INTERVIEW ABOUT?

"In general, the kinds of things that we will be asking about are the changes in your life and how your health has been in the last two years. If there are any questions that you do not want to answer, you don't have to, or if you should decide you would like to stop the interview for some reason, you will, of course, be free to do that at any point. Naturally, we would like you to give us an interview that is as complete and accurate as possible."

WHAT DO YOU WANT TO KNOW THAT FOR?
WHAT ARE YOU GOING TO DO WITH THE INFORMATION?

"The information we get from the interview should help us to understand the adjustments that people have to make in their lives when things change for them. We also want to see what effect this has on their health and well-being. Increasing what we know about these things may make it possible to help people through difficult changes in their lives."

WHERE DID YOU GET MY NAME/ADDRESS?
HOW DID YOU GET MY NAME/ADDRESS?

"We have names from several agencies, and in this study the people doing the study will not know from which agency your name was chosen. We must protect the confidentiality of our only two sources of information, the agencies we have dealt with and the people we interview. However, I can tell you that your name was selected from a list of many people who have used various kinds of services provided by agencies in ____X____ (City)."

HOW LONG WILL THE INTERVIEW TAKE?

"Some run longer than others, however, it won't be more than a couple of hours. We want to pay you for your time. We will pay you ____X____ dollars at the end of the interview."

HOW MUCH ARE YOU GOING TO PAY?

"As we said in our letter, we will pay you for giving us an interview. You will be given ____X____ dollars in cash upon completion of the interview."

WHO IS DOING THIS STUDY?

"The study is being conducted by ______X______ agency, a non-profit research organization. All the information that you give us in the interview will be used for research purposes only. Your name will not be attached to the interview, thus making it completely anonymous. Further, all the information we collect is kept strictly confidential: your privacy in this regard is protected by state and federal laws governing research studies."

WHAT IS THIS STUDY ALL ABOUT?

"Basically, the study is concerned with the changes that occur in a person's life and the effect they have on his well-being. The entire interview will take _____X_____ amount of time. It just is not possible to explain the whole thing to you over the phone: when you come into our office you will be given any further information that you want."

HARD-TO-FIND SUBJECTS

Some subjects are especially difficult to locate because of special complications in their life situation, or because of special fears about being sought. For example, a person who gives up drugs and turns instead to alcohol tends to become very mobile. Alcoholics generally move about a great deal in erratic patterns that are hard to trace. Elusive movements are characteristic of persons who are still using drugs illegally and therefore experience a constant fear of arrest.

In order to provide detailed suggestions about methods for locating hard-to-find subjects, actual case studies are included in Appendix G to this manual. These cases illustrate techniques that are useful in handling special problems: exceptional fear of arrest, alcoholism, and unusual reluctance to cooperate.

Considerations of Cost

For the researcher, the matter of reluctant subjects raises the question of when to give up. Of course, this question is related to considerations of cost-effectiveness. As noted in Chapter 3, a response rate of at least 80 per cent should be obtained in order to have meaningful results from a survey. Some refusals are inevitable, and a completion rate much over 90 per cent is probably an unrealistic goal. Interviews on the first 60 to 70 per cent of subjects will be relatively inexpensive; the next 15 to 20 per cent will cost appreciably more. But moving from 80 per cent completion to 90 per cent may cost twice as much per case as did early interviews, and every percentage point above 90 will cost perhaps three to five times what early interviews cost. A study with high recovery rates cannot be done cheaply.

PROBLEMS AFTER LOCATING: BROKEN APPOINTMENTS

Even if you are successful in locating respondents and in getting them to agree to an interview, this will not guarantee that an interview will be obtained. Broken appointments are an especially common problem in such research. In one survey of drug abusers, 41 persons out of the 286 living subjects who were eventually located broke at least one appointment. Whenever this occurred, the interviewer waited between 30 and 45 minutes. If the subject still had not appeared or telephoned to explain his lateness the interviewer then made follow-up efforts. Thus if the respondent does not show up, the interviewer or locator is able to contact him again. Generally, such a conversation would go as follows.

Follow-Up Conversations

The call would be made, and you would identify yourself and ask to speak to the subject. If the subject is not there, a message may be left stating that you had called and would call back again. If the person answering the phone knows that the subject has been in contact with you and had an appointment, he may volunteer some information about where the respondent is or when he may be home. If the information indicates the subject may still intend to be interviewed (for example, "he forgot" or "he called and said he couldn't make it"), then you would respond in a positive way, stating that you would call back again and make another appointment that was more convenient, and that the subject should not worry about having missed the appointment. ("We'll just set up another one at another time.") But if the person answering the telephone appears not to have any knowledge about you and does not recognize you when you give your name, then the only message to leave is "I will call back later."

Making Another Appointment

On the other hand, if the subject is present when you call, generally the following approach can be used:

> "This is Mr. _________ from _________ Center. I have here on my list that we have an appointment today for an interview. Now, you probably forgot all about it, so I just thought I'd call to see if you were delayed or on your way, and see if we could make the appointment again for another time when you think you can come in. Now, would tomorrow morning or Friday afternoon or Saturday morning be better times for you than today, or do you think we still have time for you to come down here today?"

Arranging for Transportation

If you suspect that transportation might be a problem, another tactic that can be used is to center the conversation around that factor. For example:

> "I thought that you might be having a problem getting a ride here, so I thought I'd call and tell you that maybe you could take a cab or someone could come out and pick you up. If you take a cab, you know, you just have them stop in front of the office and someone inside will come out and pay the cab fare for you, and we'll also pay for your cab fare back home.

The Understanding Approach

It is also a good idea to suggest plausible excuses to subjects for not keeping their appointment. You might suggest that probably something had come up at the last minute which they had been unable to foresee when they made their appointment. Working overtime, unexpected company, or illness in the family fall into this convenient category.

The emphasis in this approach to "no-shows" is to accept whatever excuse the subject wants to supply for not showing up, or to offer him an excuse if he does not volunteer one: never question whether he really intended to come in or not. Generally, your approach should be friendly and understanding. But if the person misses more than two appointments, you should take a different approach--for instance by telling him that you had to wait for him, and that his not coming meant you had to spend money that you would much prefer to spend in other ways, such as the respondent's travel expenses, refreshment, convenience, or comfort.

Changing the Interview Site

The purpose of the understanding approach described above is to allow you to complete the interview eventually. However, in a few cases it may even be necessary to change the site of the proposed interview in order to secure the interview. In one case, this might mean going to a respondent's home after he had repeatedly missed appointments. In another case, it might mean arranging to go to a drug treatment program, and waiting for him to show up for his methadone. In a third case, it might mean picking him up after work and conducting an interview at that point. In still another case, it might mean meeting him in a restaurant and conducting the interview there.

The Appeal to Reason

In the event that the subject indicates that he has had second thoughts about coming for an interview and prefers not to participate, you would shift your tactics. Instead of trying to make an appointment in the immediate future, you would talk with him about how important the study is and why his participation is needed, adding that you would like him to think about it and will call him back in a few days. Generally, this appeal to reason allows you to keep the lines of communication open, and in some cases this maneuver allows you eventually to secure the interview. In other cases, however, the subject may stubbornly stick to his guns and declare that he has decided he does not want to participate.

Chapter 7

INTERVIEWING RESPONDENTS

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Goals of Data Collection

The goals of data collection for most surveys are quite straightforward: you have defined a group of people to study, you wish to find out certain things about that group, and you have decided that the best way to get information is from the people themselves. The goals of data collection are then as follows: (1) to collect data that are accurate; (2) to collect data within the restraints of the research budget; and (3) to collect data from as many respondents as possible (discussed in Chapter 6).

Collecting Data that Are Accurate

The data collected by any survey should represent the best factual information which respondents can provide, and their most truthful opinions. To facilitate this process, you need interviewers who will help the respondents feel comfortable in the interview situation, so that they respond fully to the questions which your study is asking. The care you take in selecting and training interviewers helps in obtaining data that are of good quality.

Collecting Data Within the Restraints of the Research Budget

In general, data collection is the most expensive part of survey research because it requires coordinating the work of many people. It is important to budget realistically for good data collection. It is also important to manage the data collection effort carefully so that no money is wasted in ineffectual efforts to complete cases.

Planning for effective data collection to meet these goals is not so simple as it may first appear. The data collection procedure requires integrating the services of a number of interviewers who work independently, out of sight of those who designed the survey and will analyze the data.

This chapter suggests a number of factors which the survey administrator should consider in controlling the quality of the data collected by interviewers, within the overall constraints of the project budget.

CHOOSING INTERVIEWERS

In most interviewing of drug program clients there are two distinct tasks: (1) locating ex-clients, and (2) conducting the interviews. The skills required for these two tasks are related, but not identical. Fortunately experience shows that the people who can learn to become skillful in finding hard-to-locate respondents are usually the same people who can conduct good interviews. Experience on a number of large scale research projects suggests some characteristics which the researcher should keep in mind when hiring personnel for locating and interviewing respondents.

INTERVIEWER CHARACTERISTICS

Education

Interviewers on drug follow-up studies need to be somewhat more capable than might be true in other studies, because drug follow-up studies tend to have lengthy questionnaires that require some sophistication to administer. The interviewers need to be able to work very carefully with a respondent to obtain detailed information about his life since he left treatment, using a technique called "probing," which is discussed in Appendix J. One way to help assure that your interviewers will have the necessary ability is to require that they have had at least some college.

Personal Qualities

A good interviewer will not only be intelligent, but will also be a person who makes the respondent feel comfortable. People who are overbearing, aggressive, or hostile do not make good interviewers, and neither do people who are shy. The best interviewers are people who feel comfortable in relating to others, who are not dependent on the opinions of others to maintain their personal image, and who find non-response or hostility in others a challenge rather than a threat. But most important, interviewers should be people who will not judge the behavior of any person they interview, no matter how unusual or antisocial that behavior may be.

Matching the age and sex of respondents and interviewers does not appear to be very important. Most of the respondents in drug research surveys have been men because most of the clients of drug treatment centers have been men. However, it is interesting to note that surveys of treated clients using interviewers of both sexes have found no difference in data quality whether male or female respondents were interviewed.

Race and Ethnic Background

On the other hand, matching the race or ethnic background of respondents and interviewers is sometimes important. Spanish American respondents with a lack of facility with English may require the assistance of a bilingual interviewer. In addition, community standards and mores may make it difficult for "outsiders" to be accepted and this in turn may cause the interview to be less accurate, if it can be completed at all. For these reasons, it is a good rule of thumb to hire interviewers from the same background as the respondents. If the sample has known proportions of various racial/ethnic groups, interviewers might be hired in those same proportions and an effort made to match interviewers with respondents of the same racial/ethnic groups.

Outside Interviewers

Researchers studying drug program clients have generally used as interviewers persons trained in the treatment of drug abuse, thinking that they would have an easier time locating and getting accurate information from ex-clients. However, several large studies recently have shown that drug clients and ex-clients can be successfully located and interviewed by persons outside the treatment professions. The discrepancies between records and interviews with drug treatment clients in these studies have not exceeded, in number or kind, the discrepancies expected in interviews on any topic. Indeed, outside interviewers may even have special advantages in that the respondent may feel more certain that his responses will not get back to personnel in the treatment program as coming from him.

Knowledge of the Drug Culture

A related concern of many drug treatment researchers in the past has been that respondents might mislead an interviewer unless the interviewer were familiar with the drug culture, and were able to speak to the respondent using street language. While it is extremely important for the interviewer to be comfortable and interested in what the respondent is saying, it is not important for the interviewer to be an insider in the drug culture. When the language is not familiar to an interviewer, most respondents will translate.

On the other hand, when a respondent wishes to distort his opinions or to be misleading, even the most sophisticated interviewer can be misled: it is not possible to force anyone to tell the truth.

Inside Interviewers

If at all possible, it is advisable to use interviewers who are not connected with the treatment center. However, for financial or other reasons, it may be necessary to use staff members of the treatment center to administer interviews. In these cases, special precautions are advisable to minimize distortions in survey responses. The client or ex-client will be worried primarily about his own safety. He may distort his answers if he suspects that they may in some way harm him. For example, the respondent may fear that the interview will affect his treatment status, job, or welfare status. Since he knows that treatment personnel have connections with the program and the community, the respondent may not believe that the researcher will use the information for research purposes only. Both outside and inside interviewers must be aware of the problem of trust, but it is much more critical when the interviewer is an employee of the treatment center.

Therefore, if it is necessary to use members of the drug program staff for interviews, you might give special consideration to the possible problems and conflicts of interest arising from the following. First, the interview may repeat questions asked of the client at intake or during treatment. If he has lied previously, he may be uneasy about changing his story. Second, the interview may ask for attitudes toward the treatment center which the client may be reluctant to give to a member of that center. Third, the interview may ask about illegal behavior, such as selling drugs, stealing, or receiving welfare payments to which he is not entitled. The client may be reluctant to have such activity known to the program.

Interview Site

Some special interview procedures are set out here as examples of procedures that may be developed to minimize the danger of distorted responses where staff interviewers are used. First, if the respondent is a client, the interview should be held in a neutral area--not in the room where the respondent may have seen a counselor or gotten treatment regularly. If he is an ex-client the interview should be conducted away from the treatment center.

Reassuring the Respondent

Second, a special effort should be made to reassure the respondent: (1) that the information given in the interview will never be used to affect his treatment status; (2) that all personal identification will be removed from the document; and (3) that the interviewer is committed never to reveal anything in the interview to anyone else. If your institution has a human rights review committee, it may require that the respondent be given a statement to sign which emphasizes the voluntary nature of the interview and asks him to accept the institution's guarantee to use the data confidentially. Of course, it is crucial that these promises be kept.

Choosing Neutral Interviewers

Third, interviewers should be chosen from among those staff members who not only meet the criteria for good interviewers but who also have as neutral a relationship as possible to the clients to be interviewed. It is far better to have an interviewer who is neutral than to have one who has a strong positive image among clients. A respondent may not feel free to express his criticisms of a program to a staff member he likes and whose feelings he does not wish to hurt. Conversely, he may hesitate to speak freely to a counselor with whom his experience has been negative--especially if he imagines that the counselor is in a position to affect some part of his life adversely. Therefore, a neutral interviewer would appear to be the best selection.

Ex-Addicts as Interviewers

Researchers have occasionally used ex-addicts, who are often staff members, as interviewers on follow-up studies, in the belief that these individuals, because of their understanding of the drug culture and the addict's life, may be able to elicit more accurate information than other interviewers. However, evidence from a number of studies shows that it makes little or no difference whether data are collected by ex-addict interviewers or by others. You should apply the same criteria to ex-addicts as to other interviewers--especially those criteria emphasizing the neutrality of the interviewer and the confidentiality of data. In particular, you should be certain that any ex-addict interviewer will be able to avoid judgement and refrain from undue identification with the respondent. These tendencies will have a bad effect on the data gathered by any interviewer.

THE SELECTION AND SUPERVISION OF INTERVIEWERS

It is assumed that most programs will have to use "in-house" staff as locators and interviewers. For those programs planning to recruit interviewers from outside their staff, appropriate recruiting and supervisory procedures are presented in Appendix H.

TRAINING INTERVIEWERS

Techniques of Interviewing

There are some generally accepted interviewing techniques which are applicable to most surveys. These techniques are very different from those used in counseling or personnel interviewing, so that even interviewers who have had considerable training in these areas will still need to have additional training. These techniques are described in detail in several training manuals of major survey research groups.

Training Materials

The University of Michigan Manual is available from the Publications Office, Institute for Social Research, University of Michigan, Ann Arbor, Michigan 48106. The National Opinion Research Center (N. O. R. C.) manual is available from that agency's library, 6030 South Ellis Avenue, Chicago, Illinois 60637. Both of these manuals cost under $10. 00, and they can be used as a basis for interviewer training. Centers with sufficient funds may even wish to use one of such academic survey groups as consultants in training. A list of organizations which have active field staffs and train many interviewers each year is included in Appendix I. Some sample training materials for the interviewing and locating of respondents have been included in Appendices J and L.

Principles of Interviewing

Some of the most important principles of interviewing have been summarized here as a guide for those researchers who may wish to undertake their own training program for interviewers, possibly through actual training classes, discussed later in this chapter.

The Interviewer's Manner

It is important for an interviewer to relate to the respondent in a manner that is pleasant, but neutral and businesslike. The exchange between interviewer and respondent should not become social. For example, a respondent may want to tell an interviewer about his problems, but the interviewer should not become personally involved in helping him. (Chapter 5 of this manual discusses the advisability of referrals in such a situation.) If the respondent talks about subjects other than the interview questions, an interviewer should make tactful interventions into the conversation to return to the topic of the interview. For instance, the interviewer might say, "It is interesting to hear about your operation. Perhaps I can help you answer the question about your medical expenses if I reread it. . . ." Further, an interviewer must resist the temptation to accept inappropriate hospitality from a respondent. It is all right to accept a cup of coffee, but the common effort of the interviewer and respondent should be to work together to complete the interview. Anything that interferes with that process or makes it more difficult should be avoided.

Reading the Interview Questions

A second principle of interviewing is that the questions on the interview schedule must be read EXACTLY as they are written. By doing this, the interviewer is ensuring that each respondent is answering the same question. This is much more difficult to do than people usually imagine it to be. But it is important to emphasize that a slight change in the wording of the question can change its meaning altogether. For example: "When did you go to the treatment center?" is a very different question from "When did you last go to the treatment center?" Clearly, it will not be possible to make valid comparisons of the survey responses if the respondents have not in fact answered the same questions.

Probing

A third principle of interviewing involves the use by the interviewer of a technique called probing, which helps the respondent give fuller and more complete answers. Probing is a way of asking for additional information, or for a fuller description of the respondent's opinion, or for a clearer answer to a question, without influencing his answers. Probing techniques to be used by interviewers are discussed in greater detail in Appendix J.

Interviewers should first be taught these general principles of interviewing. Then they should learn the particular information they need for the survey they are administering. For example, it is helpful as a rule for interviewers to know the purpose of the questions in the survey document. They also need to have a close acquaintance not only with the flow of the survey and its "skip patterns" (where the next question depends on the answer to a previous question), and the kinds of probes that apply to various types of questions, but also with the possible problems which may arise when interviewing respondents both at the center and in the field. Some of these problems are discussed later in this chapter. This information should be communicated not only in training classes but also in written instructions which may be kept by the interviewers for their reference.

Training Classes for Interviewers

Training classes for interviewers should use the teaching methods that apply to most adult education. The classes should extend over sufficient time for ample training without night-and-day meetings: most people do not learn efficiently for more than six or seven hours a day. A sample agenda for a training session for interviewers for a drug follow-up study has been included in Appendix K. In addition, the teaching methods and techniques should be varied, so as to engage the interviewers at a high level of interest. Mathematica, Inc. has recently produced a video tape that is an excellent aid in teaching general interviewing techniques. This may be rented by writing to Mathematica, Inc., P. O. Box 2392, Princeton, New Jersey 08540.

Teaching interviewers to be comfortable in interviewing drug program clients and ex-clients may be done in a number of ways. Two such methods are represented by the following.

Practice Interviews

First, interviewers can practice interviewing each other, using the survey document, with a third interviewer observing and making notes. Discussion after the practice interview is helpful. Second, interviewers can interview people similar to the respondents in the actual survey. Some surveys have successfully recruited clients and ex-clients to participate in such training sessions as respondents. However, if this method is used, it is wise to recruit more respondents than you will need: experience suggests that there will always be "no shows," even when you are paying respondents for their time.

Street Language

As noted earlier, interviewers who are comfortable in their role are not handicapped by a lack of street language. However, it can be helpful for interviewers to have a good layman's knowledge of various drugs and their effects. It can also be helpful to know some of the common language used on the street to describe these drugs, even though this language by its nature changes rapidly. Certainly, interviewers who have an acquaintance with this language and use it easily in their normal conversation can use it in the interview situation. On the other hand, interviewers who do not ordinarily use this language will find it awkward. It follows that such vocabulary should not be included in the interviewer training sessions except to the extent necessary for interviewers to know the purpose of the questions in the survey document. The best rule for an interviewer is to use language that is comfortable and natural. Good interviewers can easily ask the respondent the meaning of any words they do not know.

INTERVIEWING

Supervision of Interviewers

Interviewers work very independently in the field. This means they can easily lose touch with the goals of the research and with some of the details they learned in training unless they have regular, rigorous supervision by the central survey office. One full-time supervisor can ordinarily work with ten to fifteen interviewers, who should be required to report to their supervisors every other day during the first part of a field period, and at least twice a week during the remainder of the data collection period. It is important for a supervisor to monitor the way an interviewer is working in the field not only in interviewing but also in locating respondents. Suggestions for the supervision of interviewers have been included in Appendix H.

The interviewer is usually assigned interviews after the training has been completed. If the interviewing is to be done in the treatment center, the main problem is scheduling. However, if interviewers make contact with respondents outside the treatment center, they must use many other procedures to complete the interview, in addition to the interview techniques applicable as well to center interviews.

Interviewing in Centers

As mentioned previously, it is desirable that interviews take place in rooms not usually used for treatment of clients. It is essential that they should be completely private. In addition, the room should be arranged so that the interviewer and respondent are comfortable, with good lighting, a table, and chairs placed so that the respondent can sit opposite the interviewer. In this way the respondent is not tempted to read the questions before they are asked. Ash trays, and a cup of coffee or soft drink, may help nervous respondents to relax.

Interviewing is a tiring activity because it requires total attention by the interviewer. Thus, when interviews are scheduled in the treatment center, there should be a reasonable amount of time between them. For example, if the interview takes about an hour to complete, interviews should be scheduled two hours apart. The interviewer needs time to move around and unwind, and time to edit one questionnaire before beginning the next. If you are using an hour length interview, it is probably unreasonable to expect anyone to complete more than four interviews a day.

Unlike the situation with field interviews, interviewers who work in the treatment center ordinarily do not have to persuade a respondent to participate in the interview: it will be accepted by the client as a part of the treatment process. However, this presents its own special problems, because the client will bring the same attitude to the research interview that he brings to the treatment process as a whole. A well designed interview schedule can help alleviate this problem. The interviewer can also help by emphasizing the research purpose of this interview. The following is an example of the kind of introduction that could be used:

> I'm ___________________. You are one of (100) clients selected at random to participate in a research study. The study is completely separate from your treatment, and the information you give me today will never be seen or used by anyone who treats you here. Let me show you the questionnaire. Notice that your name does not appear on it. As soon as we finish, I will check a list that shows you have been interviewed, but no one will be able to connect what you tell me to you, individually. The results of these interviews will be reported in statistics such as "80 per cent of the clients here would like more vocational training."

Such an introduction should help the client distinguish the purpose of this interview from the purpose of treatment interviews. It also helps to put respondents at ease and to calm possible fears about adverse repercussions which might arise if they responded fully.

Interviewing in the Field

Interviewing in the field requires several additional steps: (1) choosing a location for the interview; (2) contacting and locating the respondents for an appointment (see Chapters 5 and 6, and Appendix F, G, and L; (3) conducting field interviews; and (4) editing the completed questionnaire.

Choosing a Location for the Interview

Before contacting the respondent initially, the interviewer should have a neutral place in mind where the field interview could be conducted. Many respondents will suggest that their home be used, and this is fine as long as the interview can be held there in privacy and without distractions. For example, a young man living temporarily with his mother might find it difficult to answer some kinds of questions if she were within earshot. As an alternative to the respondent's home, the interviewer could arrange space in a local school, library, or medical center. In addition, many restaurants are empty between meal times. If the weather is warm, a park bench is possible, as is the interviewer's car. The essential condition is that the place for the interview must be private, and the respondent at ease.

Contacting Respondents

In Appendix L is an excerpt adapted from an interviewer manual that was used in a successful follow-up study of drug clients. These instructions summarize the manner and style that are desirable for field staff to use in contacting respondents, their families or their friends. Further discussion of this subject may be found in Chapter 6. You may find it appropriate to incorporate such instructions within the training classes for interviewers.

Interviewers may also be given a written copy of such instructions to which they may refer as they contact and interview each respondent.

Some agencies may prefer to separate the locating and interviewing functions. For example, you may wish to use ex-addict counselors to locate research subjects and other personnel to conduct the actual interview. Other agencies may wish to have the same staff member do both tasks. For more detailed discussion of how to locate subjects and protect their privacy, see Chapters 5 and 6.

Conducting Field Interviews

The interviewer should be aware that several problems may occur while trying to complete field interviews. First, the respondent may not keep an appointment. People who are using drugs are often not very concerned about time and commitments. If the respondent fails to show up for an appointment it may only mean that he lost track of the time. To minimize missed appointments, interviewers should ordinarily meet a respondent at his home, and then take him to the place chosen for the interview, if his home does not offer sufficient privacy. It is a good practice for an interviewer to call the respondent before leaving for the interview, and to arrive early. If the respondent breaks the appointment, the interviewer should make another one immediately. After several broken appointments, the interviewer should conclude that the respondent is avoiding the interview. A supervisor should help decide on the next strategy from that point. Chapter 6 of this manual suggests some additional tactics that may be used to deal with the problem of broken appointments.

Second, sometimes an addicted or alcoholic respondent will not be able to complete the whole interview schedule at once. (See the case studies in Appendix G.) An interviewer should be alert to this difficulty and offer to complete the interview later that day or another day. Finally, the respondent may refuse to begin the interview at all. If this situation arises, the interviewer should be very careful to leave before the conversation becomes argumentative or hostile. Another interviewer may be able to persuade the respondent to complete the interview schedule at some other time.

Procedures to be used during the actual interview have been outlined earlier in this chapter in the section on "Training Interviewers."

Editing the Interview Schedule

Once the interview is completed, the interviewer should edit it while the memory of the interview is still fresh. Editing is the process of checking each question to be sure that the numerical codes representing the answers are clearly circled, written answers are legible, and abbreviations are spelled out so that they will be understandable to the whole research team.

The following editing procedures are advisable. If the interview was conducted in pencil, the editing should be done in ink, and vice versa. No interviewer should ever enter an answer based on what he thinks the respondent would have said. Nor should he ever erase an answer, either during or after the interview. The errors made by an interviewer using this kind of "judgment" are much greater than those he may make by recording incorrectly during the interview.

After editing, the interview schedule should be returned to the interviewer's supervisor.

INTERVIEWER SAFETY

The Need for "Savvy" Interviewers

Apart from certain dangerous areas in some large cities, interviewers are probably as safe contacting and interviewing former drug addicts as in most other studies. However, persons having to work in the poverty areas of a large city should have the experience necessary to keep themselves safe. Many good interviewers have this savvy, even though they may never have been connected with the drug culture, because their life experiences have provided them with the knowledge necessary to make good judgments about where to go and how to behave. It is important to have such people on the interviewing staff for any survey, but this is especially important for surveys of drug users. The only way to determine whether an individual is likely to have the right background for this kind of interviewing is to state the matter plainly during the recruitment interview and listen to the response. If the applicant has lived in such areas or spent a good deal of time there, and does not feel afraid, he is probably safe.

Guidelines for Interviewer Safety

Some guidelines may also help interviewers to maintain their safety. First, interviewers should state their business clearly and often, and back it up by showing their identification so they will not be confused with drug dealers or with government agents of one kind or another. They should never seem mysterious. Second, interviewers should have a body image that is clearly open. Areas of the body that can hide weapons should be exposed frequently, and brief cases should always be available for a look by a nervous citizen. One very successful interviewer, to show that he is not a threat, always wears his coat open, removes his jacket when possible, and stands away from doors so he can be clearly seen through the keyhole. Third, in most cases, interviewers should share the racial/ethnic characteristics of the area in which they work.

QUALITY CONTROL

With adequate supervision in the manner described previously, most interviewers are honest and will complete their work according to the specifications the project director gives them. However, from time to time an interviewer may take short cuts in completing an interview. On rare occasions, an interviewer may systematically "cheat" by completing only parts of interview schedules or by falsifying entire interviews. Clearly, the researcher should try to protect the project from these kinds of quality losses. The usual way to do this is to subject a proportion of each interviewer's work to a process called validation.

Validation of Interviews

Validation consists of contacting some of the respondents again later, to verify that their interviews actually took place. It often includes asking the respondent a few questions from the interview to verify that the interviewer did not skip sections of the questionnaire. This kind of validation of interviews with drug treatment clients may not be desirable when the interviews are completed at the center. The fact that the interviewer and respondent spent some time together in a room and emerged with a completed interview is validation that the interview actually took place. However, it is advisable to validate interviews conducted in the field. Interviews to be validated should be selected at random and should consist of 10 to 20 per cent of an interviewer's work.

Because treatment programs using their own staff as interviewers may not feel the need to use these procedures, the detailed discussion of how to conduct them has been placed in Appendix H, on the selection and supervision of interviewers. However, many program directors may be well advised to use these procedures even with in-house staff.

It is also a good procedure for a member of the office staff to review all interview schedules to determine whether there have been skipped questions and errors in recording. These errors should be reviewed with the interviewer on a regular basis. Toward the end of a study, as the interview load declines, it may be possible to reduce the size of the interviewing staff. The best way to do this is to release those interviewers who have produced the fewest interviews, at greater cost per interview, and those interviewers whose rate of errors is high, as judged by editing. Studies have shown that inefficiencies in one area, such as cost, are often correlated with lower quality and low productivity.

CONFIDENTIALITY AND PRIVACY

Herman Melville wrote that a confidence man is a person in whom one has confidence. In this sense, it is desirable for all interviewers to be confidence men. One of the first tasks in interviewing respondents who may be suspicious of research, of strangers, or of the "establishment" in general, is to convert the respondents to a sincere belief in the validity and honesty of the interviewer and the research. Only in this way will the best data be collected. At the same time, however, the interviewer must be a person in whom another kind of confidence can be reposed. Even after the researcher has taken all the precautions that are possible to protect the respondent's confidentiality, there still remains one danger: leakage of project information.

Discussion of Interviews

To minimize this danger, interviewers should be trained not to talk to anyone about their work--not even to family and friends. Most survey groups regard a breach of this rule as grounds for dismissal. However, because it is difficult for the interviewer not to be able to discuss interesting interviews, the time for supervisor-interviewer conferences should include an adequate amount of extra time, in which the interviewer can just talk about his experiences.

Chapter 8

INSTITUTIONAL SOURCES OF DATA

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A vast body of information concerning your former clients already exists, even before your field work begins. This information exists in drug treatment programs, welfare agencies, schools, employment offices, and enforcement and correctional facilities. In addition, it is available in federal, state and city agencies such as social security administrations, state employment agencies, and city narcotics registers. The institutional data discussed in this chapter include all those records maintained by local, state, or federal agencies and organizations which bear on the functioning of drug treatment clients.

There are two types of institutional data: (1) those describing the general population and populations that are similar to your treatment populations; and (2) those describing your specific research subjects. This chapter discusses these two types of data in this order. Institutional records should be differentiated from the information gathered directly from the clients in the follow-up.

In planning a follow-up study of drug abuse clients, it is necessary to consider the available sources of relevant institutional data in order to decide which data sources to employ. Obviously, it would not be feasible to collect all relevant information which such institutions possess concerning their past, present and future contacts with the study sample. Therefore, it is necessary to categorize these institutional sources and to consider what data might be sought from each in a particular follow-up project. After reviewing this chapter, you may decide that seeking information from outside agencies is worth whatever cost and difficulty may be associated with it. Or you may decide to restrict your efforts to particular institutional records. However, to make such a decision knowledgeably, you must first familiarize yourself with the kinds of data that might be available and the procedures necessary to secure them.

SOURCES OF AVAILABLE DATA

Several general sources of data may be relevant to follow-up studies of drug abuse clients.

First, there are the records of the treatment program undertaking the follow-up study. These records commonly contain admission information, reports of medical examination and treatment, progress notes of counselors and administrative information concerning various aspects of the client's stay. Presumably all follow-up studies will use some of this information.

Agency Records on Study Subjects

Another source of information about the study population consists of records of other institutions--medical, social service, educational, law enforcement, alcohol treatment, drug treatment, and so on. The usefulness of their records will depend upon the objectives and intended thoroughness of the follow-up study, as well as upon the characteristics of the drug abuse subjects selected.

Institutional Data Regarding Other Drug Abusers

A third source of information--often overlooked--consists of published national and regional statistics on drug treatment populations, with which your own study's findings can be compared. For example, it may be relevant to know about the typical criminality of populations of drug abusers nationally, in order to judge whether your own program's clients are an unusual group. So too with regard to educational attainment, employment and drug use. In addition, it may be valuable to consider how the clients selected for study differ from the general population of drug abusers in treatment. Researchers have now delineated not only the general characteristics of the 250,000 drug abuse clients in treatment in the United States but also the specific characteristics of discrete subpopulations of clients. These studies make clear that drug abuse clients are not a homogeneous population, and that different types of programs serve different clients. A major source of data regarding clients in treatment in programs throughout the country is maintained by the National Institute on Drug Abuse through the Client Oriented Data Acquisition Process (CODAP). Inquiries with respect to this file may be directed to the Division of Scientific and Program Information at the National Drug Institute on Drug Abuse. Some of these data are presented in Volume II of A Manual for Drug Treatment Program Self-Evaluation, entitled Table of Values *on Selected Treatment Outcome Measures Based On Data From the Drug Abuse Reporting Program (DARP)*, available from the Clearinghouse, National Institute on Drug Abuse.

Census and Demographic Data

Finally, another potentially important source of information is to be found in census data and vital statistics for your local area. Knowing the unemployment, crime and mortality rates in the follow-up locale makes it possible to compare the study subjects with the general population. Data regarding employment or unemployment shown by age, sex and race are available for selected Standard Metropolitan Service Areas (SMSAs) through the Bureau of Labor Statistics, Department of Labor, 441 G Street, N.W., Washington, D.C. 20001. In addition, aggregated data on crime are often available from local police forces. Data regarding mortality can be obtained from local and state offices of vital statistics. Descriptions of the population in the communities under study by age, sex, marital status, and occupation can be obtained from the Bureau of Census for relatively nominal sums. Such data are available by census tract and blocks for large urban communities.

THE VALUE OF INSTITUTIONAL DATA SOURCES

There are three principal reasons for seeking information from institutional sources in follow-up studies of drug abuse clients: (1) to note recent addresses to help in locating clients for interview; (2) to check the validity of information obtained from prior records and interview data; and (3) to obtain additional information about the study subjects.

Updating Search Sheets

Whenever a subject is located in an agency file, any new information in that record about aliases, addresses, relatives or employers should be entered and dated on the search sheet. When interviews and record data disagree, it is not necessarily the case that the subject was lying in the follow-up interviews. Records are often simply reports of previous interviews with that subject in a context which may have made him less likely to be completely candid.

Independent Measures of Validity

It is necessary to consider what evidence there is for the accuracy of all types of available information. This issue needs emphasis because of the inherent limitations of relying exclusively upon any one source of information, whether interviews, questionnaires, medical records, or other institutional sources. Each of these sources has strengths and weaknesses. Thus, institutional records can corroborate or expand upon the information provided through interviews. Conversely, competent interviews can provide detailed information not otherwise available. Taken together, the institutional records and personal accounts provide more complete and valid data about follow-up clients than would be obtained from a single source.

Additional Information

The third general reason for using institutional records in follow-up studies is to obtain additional information about the subjects, such as employment history prior or subsequent to treatment, criminality, schooling, and other experiences the subjects have had with local institutions. The adequacy of these secondary institutional data concerning a particular drug abuser population in a given community varies considerably. In all instances project personnel will have to take care that the data under consideration are not only accurate, but also that they have been updated recently. The adequacy of the data available will depend primarily upon how old the records are and how professional the health, welfare and/or police agencies are in the area served by your treatment program.

GAINING ACCESS TO INSTITUTIONAL DATA

In planning to obtain data from institutional sources concerning former clients, it is first necessary to consider how to approach the particular health, welfare, educational, police or penal institution in a way that will enhance the likelihood of their cooperation. It should not be assumed that these or other agencies will automatically or easily provide outside researchers with confidential information. Indeed, the opposite may be the case. Most agencies are quite careful about releasing information concerning the individuals they serve or with whom they have contact. They will have both ethical and legal issues to consider in deciding whether to provide the information you want.

Developing a Strategy for Cooperation.

Since it involves gaining the support and cooperation of local officials and agency directors, gaining access to institutional data requires tact, honesty, and perseverance. If you should decide to seek such data, you would be well advised to develop a plan in advance for securing the cooperation of those persons in charge of the data.

You might begin by having the head of your agency enter into negotiations with the head of the agency whose records you need. For locally funded programs in particular, it may be useful to rely on the good offices of the mayor or city manager to aid in this negotiation. Once an agreement to cooperate has been obtained, further negotiations can then go forward between your research director or his agent and the head of the record room in the other agency.

Principles in Working with Agency Personnel

You will probably need to give the agency officials a guarantee that the information they provide will not be used against them. In any case, you should assure the agency that only tactful and responsible staff members will be sent to deal with their agency. It is unreasonable to expect an agency director to provide you with confidential data if he fears that this information may be used by you, or others, to discredit him or his agency. Indeed, it may be appropriate to indicate that some benefit will accrue to the agency that provides you with data. Such a benefit might include written recognition in the study report, or providing the agency with those research findings that may be useful to its staff. In any case, reports produced and data obtained should be made available to cooperating agencies. On no account, however, should the researcher make or accept an offer to exchange information about individual clients in an effort to gain access to institutional records, nor should any former client be identified as a drug abuser to someone who may not know that fact. (See Chapter 5.)

To facilitate initial and subsequent cooperation, it will be appropriate if not essential to demonstrate that your study will benefit both drug abusers and the community. However, you should recognize throughout that agency officials are, or will likely see themselves as, "doing you a favor." By and large, the agency staff will be performing extra work for which they are not compensated in order to provide you with the data. In fact, it may be advisable to offer to pay the agency's staff for the extra effort required, if you have the funds.

Informal Contacts

You should also recognize that not all negotiations between treatment programs and agency staff are conducted on a formal level. The data gathering staff and the staff at the data source may have friends in common who can act as intermediaries by vouching for the treatment program. The treatment staff might consider the possibility of seeking the help of these informal contacts in addition to working through the formal system.

SELECTED SOURCES OF PARTICULAR TYPES OF DATA

The alternative sources for several specific types of data will be discussed next, as well as some of the issues you should consider in securing data from them.

Arrest Data

With regard to arrest data, there are four basic sources: (1) juvenile court records; (2) records at the local police precinct and at the central police headquarters; (3) local jail records and correctional department records; and (4) court records. Juvenile court data are typically difficult to acquire because of the special concern with the confidentiality of records of juveniles. Nonetheless, these data constitute a very significant proportion of the criminal records of a population as young as are most drug treatment clients.

As noted in several earlier chapters in this manual, identifying information should be collected at intake into the treatment program that can later be matched up with institutional data, such as date of birth, social security number, and all names and aliases ever used by the individual. Obviously, care should be taken to verify the identity of cases appearing in criminal justice records, before assuming they are your clients. In some instances clients may already have an identifying number from the court or correctional agency, as in the case where referral has been made directly by one of these agencies. This will make the records search easier.

Individuals selected to collect the necessary data should be carefully chosen: they should be non-offenders themselves, they should have appropriate concerns about client confidentiality, they should dress appropriately and they should act with sufficient reserve. Training in behavior appropriate to data collection at the agencies in question should not be neglected. Only tactful and responsible staff members should be used. In addition, the project investigators should be concerned at all times about the confidentiality of their own client records and names, providing an additional reason for careful selection of the individuals gathering the data.

Particular care should be taken with regard to information collected from jail units. Check to determine whether only felony cases are referred to the jail, since the disposition of other cases may be made at the police precinct level. Also check to determine whether or not the charge recorded is the charge actually made against the individual. With criminal justice data, there may be considerable delay between the time of an arrest and the appearance in the main file of information about the disposition of the case. Therefore, a return visit after some months is advisable to determine whether new information has appeared.

Employment and School Data

Data with regard to school and employment history are apt to be limited. In fact, recent federal legislation prohibits schools from disclosing the records of their students without the written consent of the student (or his parents, if the student is a minor).

Two additional sources of employment data that at least potentially are available to government-sponsored treatment programs consist of those data maintained by the Social Security Administration and the state unemployment compensation offices. Social Security earnings records can not be made available for specified clients, but will be reported to programs on an aggregate basis only.

Drug Abuse Data

The gathering of material with regard to deaths of former clients from the medical examiner's or the coroner's office is usually easy to obtain. The program staff should be sure to record the cause of death, since drug-related deaths are one important measure of treatment failure. It is also possible to have vital records departments make searches if you suspect that an individual may have died in another community. You must provide not only name and birth date, but also year of death, and if possible, the precise date. In contacting selected state vital records offices, the treatment program may make use of Publication No. HSM-72-1142 (Where to Write for Birth and Death Records), which is available from the Department of Health, Education and Welfare.

Other potential sources of information on illicit drug use would include the records of hospital emergency rooms, other treatment agencies, social services agencies and mental health facilities. These agencies sometimes are extensively used by drug abusers, and appearance in these records, like appearance in arrest records, indicates the client is having problems. Health and social agencies are, of course, zealous about protecting the confidentiality of these records. Access to them, like access to school records, will require written permission from your subject.

Chapter 9

DATA PREPARATION AND DESCRIPTIVE ANALYSIS

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Next Steps

Once you have completed the interviewing and search of relevant records, it will be necessary to organize your information for presentation in the form of charts, tables, and written summaries. The procedures described in this chapter are appropriate for smaller programs where the information can be hand-tabulated. However, these procedures also have to be performed where the sample size is large and where you plan to use tabulating machines or a computer. At a minimum, the coding procedures will have to be carried out prior to putting the information onto IBM cards or computer tapes. All the analytical and tabular methods described here can be carried out by computers. However, you must first understand what it is you want to do with the information. Computer technicians are available wherever there are computer facilities, but such personnel are only helpful when you can specify what you want. Therefore, it is important to have a clear understanding of the basic methods used to organize data.

CODING

In general, respondents are asked the kinds of questions that appear in the sample interview schedule in Appendix E.

Simple Data Categories

Some data, like sex, can only be organized reasonably in one way--male, female. Other data, like age, need to be grouped into larger intervals. Otherwise you will have too many groups to make intelligible tables. These simple categories of data are very useful in any analysis, and this chapter describes how to make some of the decisions about their use.

Multidimensional Variables

A second class of variables will develop from such interview questions as "What was the title of your last job?" You may want to use the elicited information in several ways. For example, you may want to determine what skill level was associated with the job. Or you may be interested in the relative social status or prestige associated with the occupation. Because these two aspects of employment are not always redundant, you may not wish to limit your data analysis to one dimension. This chapter describes various ways of coding such data to preserve their multidimensional significance.

Uncoded Data

When the interview includes open-ended questions for which you have not developed answer categories ahead of time, the interviewer records the response in the client's own words in order to discern what might be salient for him/her. Such questions present a variety of problems for the analyst and should be avoided if you can identify meaningful categories in advance, from which the respondent can be asked to make a choice. Ways to develop these categories are suggested in Chapters 2 and 4. Where this is not feasible, you will have to review the answers you get after interviewing is completed, and sort the ansers into a few code categories before they can be used in analysis. This chapter discusses some of the principles that can assist you in developing useful schemes for such data.

GROUPING CATEGORIES

When analyzing the interview data, you will observe that some data, such as the respondents' ages, are distributed over a large number of values. The distribution will vary with the type of program and the target population served. For example, you may find that in the first ten interviews you examine, the ages are as follows:

Age

19, 27, 24, 17, 29, 28, 21, 21, 22, 19

The first task is to order this information sequentially from low to high, as in the following illustration:

17, 19, 19, 21, 21, 22, 24, 27, 28, 29

Frequency Distribution

It is apparent from the data that several of the respondents are the same age. You could group these data into a frequency distribution as follows:

Age	Frequency
17	1
18	0
19	2
20	0
21	2
22	1
23	0
24	1
25	0
26	0
27	1
28	1
29	1

Your next goal is to find a grouping of the ages which retains sufficient discrimination so that the grouping does not obscure important age-related trends, but which has enough cases in every category to get a stable estimate of the frequency of outcomes for respondents in that category. For some kinds of data, there are conventional categories, such as five or ten year intervals in the case of age data. Using such categories is especially useful if you wish to compare your results with those of other programs or with census figures. Or suppose that you wish to examine the relation between age and employment. You know that younger persons have a harder time obtaining jobs. Therefore, you might group ages in a way that reflects your experience with job opportunities. For example, you might sort out teenagers, younger adults, and more mature clients as follows:

Age	Frequency
17-20	3
21-24	4
25-29	3

This grouping illustrates two principles. First, the data should be grouped into categories that have conceptual importance for the use you plan to make of the information. Second, there should be roughly similar numbers of cases in each category. Sometimes the first consideration will inhibit the second one, as with a program which has only a few clients who are well advanced in age.

Of course, the above example has too few cases in each category to let you draw reliable conclusions. Subsequent sections of this chapter illustrate methods of data analysis using more realistic numbers.

MULTIDIMENSIONAL CODING

This refers to the use of the same information in several ways to answer different questions. For example, if the interview schedule asked for the respondent's job title, you might obtain the following enumeration of categories:

- cashier
- machinist
- plumber's helper
- clerk in a store
- bookkeeper
- salesman
- tool maker
- laborer

If your interest were in the status implications of the jobs, you might classify them into those that are white collar as opposed to those that are blue collar:

White Collar

- cashier
- clerk
- bookkeeper
- salesman

Blue Collar

- machinist
- plumber's helper
- tool maker
- laborer

On the other hand, you may want to focus on the level of skill each occupation represents so that you can determine whether clients who have acquired higher levels of competence find employment more readily upon completion of treatment, irrespective of the status implications of their jobs. For such purposes, you might classify the jobs as follows:

Skill Level

• High	Machinist, Plumber's helper, Bookkeeper, Toolmaker
• Medium	Cashier, Salesman
• Low	Clerk, Laborer

OPEN-ENDED QUESTIONS

As discussed previously, where you record responses in the clients' own words it is desirable to reduce the responses to a small number of categories that still retain most of the meaning in the answers. Developing such categories depends not only on the questions you asked, but also on the quality of the responses you succeed in eliciting. As an example, if the question asked the clients what they liked about the program, you might elicit the following responses:

- The program gave me a chance to get my head together.
- The chance to talk over my problems with other people who have been through the same thing was the best part of the program.
- I needed to get away from my family so they couldn't hassle me any more.
- I got a chance to find out that I could do different kinds of things as well as the next guy.
- The way the staff treated me as a person and not just another junkie.
- The job training program was most useful.
- I liked the educational program because I learned to read better.

Developing Codes for the Data

To summarize this information in tabular form you need to develop a scheme of mutually exclusive categories. The scheme should contain a rationale that is helpful in the analysis. In developing categories for the responses used as an example above, the primary interest might be in the features of the program that are spontaneously reported as helpful. However, some responses refer to specific features such as job training, while others are much more general, such as being able "to get my head together." That is, some individuals refer to their personal experiences with the program, while others allude to particular program attributes they found helpful, such as job training. If these issues are your central concern then you might develop categories along the following lines:

- Personal Needs:
  - Get head together
  - Get away from my family
  - Chance to get to do things

- Program Attributes:
  - Staff
  - Group therapy
  - Individual counseling
  - Job training
  - Education

Broader Coding Categories

A more general scheme, applicable to a variety of open-ended questions, would distinguish three broad elements of any motivation. Thus, the question "Why did you come into this program?" might elicit three distinct types of responses.

First, some respondents may attribute their entry into the program to various influences such as police or court pressure, or family members encouraging the individual to seek help. Second, some clients may have been primarily motivated by the fact that they were in a situation where they could no longer function, or felt that they must change. These client needs might include a wide range of responses, such as "My veins gave out," more general health considerations, or references to a strongly felt need for personal change by the applicant. Finally, some clients may mention things they heard that made them choose this particular program rather than another -- for instance, that they knew the program provided methadone for detoxification.

Subcategories

Three aspects of motivation may be identified in these responses: (1) environmental influences; (2) client needs; and (3) program qualities. Within each of these broad categories you can develop a variety of subcategories if there are enough responses to justify more refined classifications. For example, subcategories under "client needs" might include distinctions among considerations such as: (a) health; (b) personal adjustment; and (c) other personal needs.

The precise subcategories will differ among programs combining different modalities and drawing on different populations. But such a scheme might look something like this:

Environmental Influences	Client Needs	Program Qualities
• Family	• Health	• Location
• Friends	• Social Adjustment	• Residential Program
• Criminal Justice System	• Personal Change	• Drug-free
• Mass-Media	• Drug Shortage	• Medical Facilities
• Ex-clients		• Skills Training
• Out reach		• Therapy

For some purposes you might use only the broader categories, but where the sample size is large enough, you may want to tabulate the subcategories as well. In addition, note that an individual may fall into more than one category. As suggested in Appendix E, you need to assign a separate column on the punch card for each category if this is a possibility. Classifying answers according to such a rational scheme reduces the number of responses and is also potentially more useful for understanding why clients enter treatment.

These schema are only illustrative. It may be useful to develop very different ones, depending on the way the questions are asked and the purposes of the analysis. You should avoid schemes that are too complex, because they will severely limit the types of analysis which can be done.

Consistency in Coding

The classification scheme must also be simple enough so that different coders, whose job it is to classify the responses, can agree on the classification assignments. Their level of agreement can be evaluated by having two or more individuals code the same responses independently. A good rule of thumb is that you should obtain at least 80 per cent concurrence between coders in the classification of responses. A lucid discussion of "accounting schemes" can be found in Zeisel (1968).

ORGANIZATION OF THE DATA

The information necessary for the analysis is likely to come from various sources, such as intake records, counselor reports, urine reports, follow-up interviews, police and other records. Once the decisions are made on the information to be included, it is important to organize it in a form that facilitates easy handling.

The Data Card

The complexity of the data reduction scheme needed will depend on the number of subjects and the number of items of information per subject. If both are small, it may be sufficient to place all the information about one respondent on a single 5" x 8" card, and simply count the items on each card by hand. If more subjects or more information per subject is obtained, it may be necessary to employ tabulating equipment, and transfer the information to IBM cards. In this case, outside consultation may be worthwhile. But before the information can be placed on IBM cards (or, eventually, on computer tapes), it will still be necessary to integrate the information in a coherent form, generally on a single document such as a "coding sheet," to facilitate transfer to IBM cards.

Where a 5" by 8" card or similar document is used, several things should be kept in mind. First, it is necessary to have an identification number for each case. This should not be the subject's name, nor a number which discloses his identity, such as a social security number or clinic number, as explained in Chapter 5. Second, the same variable should be located in the same place on each subject's card or form to facilitate tabulation. Third, the exact age or year of birth should be recorded, rather than reducing the information to broad age-ranges on the coding record. It is better to collapse information during analysis, after you see how the frequencies are distributed among categories.

Layout of the Data Card

In a follow-up study the general layout of the data card may be structured around several basic areas, as illustrated in Figure 1.

FIGURE 1

Example Of A Possible Coding System For A Follow-Up Study Of Treatment Clients

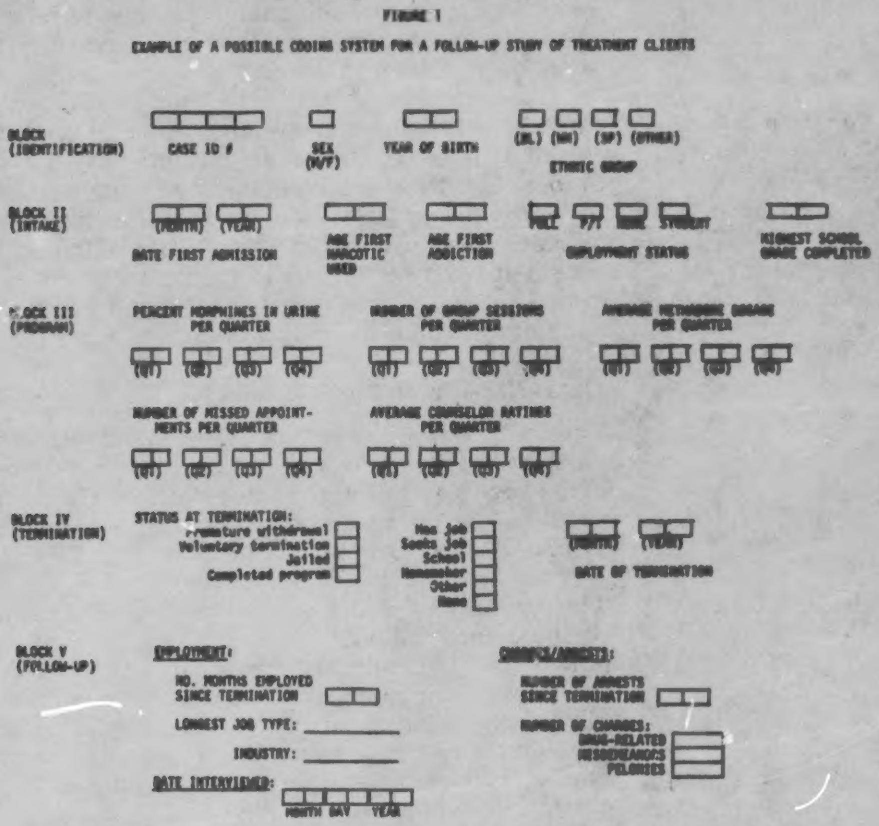


FIGURE 1
EXAMPLE OF A POSSIBLE CODING SYSTEM FOR A FOLLOW-UP STUDY OF TREATMENT CLIENTS
BLOCK (IDENTIFICATION)
CASE ID #
SEX (M/F)
YEAR OF BIRTH
(BL) (WH) (SP) (OTHER)
ETHNIC GROUP
BLOCK II (INTAKE)
(MONTH) (YEAR)
DATE FIRST ADMISSION
AGE FIRST NARCOTIC USED
AGE FIRST ADDICTION
FULL P/T NONE STUDENT
EMPLOYMENT STATUS
HIGHEST SCHOOL GRADE COMPLETED
BLOCK III (PROGRAM)
PERCENT MORPHINES IN URINE PER QUARTER
NUMBER OF GROUP SESSIONS PER QUARTER
AVERAGE METHADONE DOSAGE PER QUARTER
(Q1) (Q2) (Q3) (Q4)
NUMBER OF MISSED APPOINTMENTS PER QUARTER
AVERAGE COUNSELOR RATINGS PER QUARTER
BLOCK IV (TERMINATION)
STATUS AT TERMINATION:
Premature withdrawal
Voluntary termination
Jailed
Completed program
Has job
Seeks job
School
Homemaker
Other
None
(MONTH) (YEAR)
DATE OF TERMINATION
BLOCK V (FOLLOW-UP)
EMPLOYMENT:
NO. MONTHS EMPLOYED SINCE TERMINATION
LONGEST JOB TYPE:
INDUSTRY:
DATE INTERVIEWED:
MONTH DAY YEAR
CHARGES/ARRESTS:
NUMBER OF ARRESTS SINCE TERMINATION
NUMBER OF CHARGES:
DRUG-RELATED
MISDEMEANORS
FELONIES

First, it should include the basic identifying information which must appear on every card -- the study number, card number, and case number. Then there are demographic data, such as age, sex, and race. A second block might consist of intake variables, such as date of entrance into the program, age of first narcotic use, and age of first addiction. The third block might consist of program indicators, such as counselor evaluations, methadone dosage, consistency of participation, or number of positive urine reports. The fourth block might show the status of the client at the time treatment terminated, in terms of reason for termination and his drug use and adjustment at that time. A final block might consist of the follow-up information, such as arrests, employment, and current drug use.

You may not need all the information illustrated in Figure 1. However, it should be noted that the data are roughly organized along a time dimension to facilitate locating the information you may need. The information is placed in specified locations. For items like sex or status at termination it is only necessary to look up the appropriate box.

Narrative responses should be limited to as few items as possible. Both sides of a card can be used so that more information may be included. If necessary, each respondent can have two or more cards as long as the case numbers are included so that the cards for the same individual can be linked.

Where information is to be transferred to IBM cards, then the precise location that each variable will have on the IBM card should also be specified on the unitary form you use to collate all the data. (That is, a card number and column locations will have to be specified for each variable.) It will be necessary to eliminate narrative responses by coding the data before transferring them to IBM cards, as described previously.

PRESENTATION OF THE DATA

A description of the client population the program is serving is a necessary prelude to any study. The kinds of clients which a program attracts can influence almost every aspect of the program and its effects. Therefore, variables describing the clients in terms of age, sex, race, education, prior drug use, and arrest history should be included.

It is often convenient to present such data in charts and tables that readily communicate the essential information. Charts and tables should not burden the reader with excess detail that can obscure the essential facts. This chapter first reviews tabular layouts which show single variables such as age or sex or arrest histories. Then graphs are discussed, which are easier to grasp than large arrays of numbers.

TABLES

You may want to present in tabular form the age distribution of the clients who fall into your study group. If it is a program that tends to serve mainly younger clients, you may discover that most of the ages range between 15 and 34, with only a few somewhat older.

Frequency Distributions

After listing the ages in sequence you might find it more convenient to group them into five-year intervals as follows:

AGE DISTRIBUTION OF CLIENTS IN PROGRAM XYZ

Age	No.
15-19	17
20-24	18
25-29	19
30-34	12
35+	3
	69

From the distribution of the frequencies you observe that the number of clients by five-year groups is reasonably constant from ages 15 to 29, and declines thereafter. There are so few people over 35 that this is an open-ended category where every respondent over 35 is grouped together, irrespective of the exact age.

Percentage Distributions

Presentations of raw frequencies, as in the table above, make it hard to draw comparisons between programs or between one period and another for the same program. Percentage distributions allow comparisons when the number of cases differ in the groups being compared.

The table above presented as percentages would look as follows:

AGE DISTRIBUTION OF CLIENTS IN PROGRAM XYZ
(IN PER CENTS)

Age	Per Cent
15-19	25
20-24	26
25-29	28
30-34	17
35+	4
	100
	(69)

Because of the modest sample size in this example, percentages have been rounded to whole numbers to avoid a spurious sense of precision and to simplify the table for the reader. It should be noted that the total number of subjects appears in parentheses below the 100 per cent total, to allow the reader to evaluate the stability of the percentages. The larger the number of cases, the more stable the percentages. For example, if there are only ten cases, changing a single case from one category to another changes the two affected categories by 10 percentage points each. On the other hand, if there are 300 cases, changing a single observation would add or subtract only three-tenths of one per cent.

Two-Column Presentations

Some researchers prefer to present two columns: one column containing the actual frequencies, as in the first table, and another containing the percentages, as in the second table. However, when tables contain two or more variables, this practice creates very cluttered tables. In any case it is unnecessary, since the reader can reconstruct the frequencies by multiplying the total number of cases by each percentage listed in the table.

Titles for the Table

Each table must have a title that informs the reader what information is contained in the body of the table. Column headings must be clearly identified. You should avoid abbreviations that detract from communicating the nature of the information contained in the table.

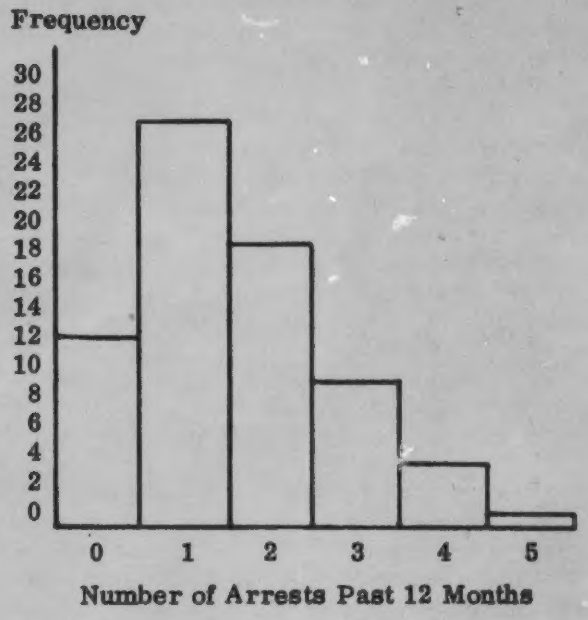
GRAPHS

It has been noted previously that a long series of numbers communicates little information to individuals who are not familiar with research. However, even experienced individuals can often learn a great deal from a graphic display of data.

Histograms (Bar Graphs)

The following bar graph presents information on the frequency of arrests in the sample. The horizontal or X-axis displays the number of arrests in the past 12 months. This is the variable that is of central interest. The vertical or Y-axis displays the number of clients who had that number of arrests. Frequencies (or percentages) are always located on the vertical axis.

Client Arrest Histories for 12 Months
Preceding Intake in Program XYZ



From this graph it may be observed that only a few (12 per cent) of the clients have had no arrests in the 12 months prior to entering treatment. The most frequent (or modal) number of arrests in that year is one, and as the number of arrests increases, the number of clients sharply declines. All of this information can also be displayed in a corresponding table. However, the trends may be more discernible when presented in graphical form.

On the Y-axis the actual frequencies can be found by simply looking across from each observation to the appropriate point on the scale. Unless the scale on the vertical axis is large, only approximations to the exact frequency can be read off. But this lack of complete precision is less important, in most cases, than the addition such graphs make to communication. It is sometimes useful to substitute percentages for the frequencies, particularly if samples of different sizes are being compared.

The histogram or bargraph is primarily useful for an ordered series, but it can also be used to present data where it cannot be assumed that one category is "higher" or "lower" than another on some scale. For example, histograms should be used to compare numbers by sex, ethnicity, or type of drug history.

SOME DESCRIPTIVE STATISTICS

Whenever there is an array of numbers -- whether an age distribution, number of months employed, or number of arrests -- it is often useful to summarize these by a single number that reflects the overall experience of the group. Descriptive statistics are simply some convenient procedures for summarizing data. These are basically simple concepts. However, erroneous conclusions may be drawn if some of the factors that influence them are not understood. Therefore, this chapter reviews some of the basic assumptions behind these statistics.

The Mean

The mean -- or average -- is widely used and understood, and it is therefore particularly valuable. It should only be computed on ordinary numbers -- which in principle can include a zero, and for which a "two" is twice the size of a "one." Such numbers would represent counts of missed appointments, dollars earned, or milligrams of methadone prescribed. Where ratings are used it is technically incorrect to use the average, because you do not know that a rating of "3" meaning "much improved" is three times the size of a "1" meaning "no change." However, this rule is frequently violated even by experienced researchers. The mean cannot be used at all to describe variables that have no inherent order, such as ethnic classification, types of criminal charges, or sex.

Computing the Mean of Single Numbers

If you have a series of ages of clients in the program and want to compute the mean, you simply total all the ages and divide by the number of subjects. If you have the same age distribution you had in the first section of this chapter, you would find the total -- 227 -- and divide by 10, the number of cases. The mean would be 22.7 years of age.

The formula for the operations just performed is:

$M = \frac{\Sigma X}{N}$, where M = Mean
Σ = Sum
N = Number of cases
X = The individual's value on the variable in question (e.g., his age)

Sometimes the symbol $\overline{X}$ is used, instead of M, to stand for the mean.

Computing the Mean of Frequency Distributions

Often, however, your data are in a frequency distribution, rather than in an array of single numbers, as illustrated in the first section of this chapter. In such a case, the formula is modified slightly to facilitate calculations:

$M = \frac{\Sigma f X}{N}$, where f = frequency

If you look back at the previous frequency distribution you will notice that there are two cases at age 19 and two at 21. The symbol "f" simply reminds you that you have to multiply each particular age -- such as 19 -- by the appropriate frequency, which in this case is two. The resultant answer is exactly the same as in the first formula.

If your data are in a frequency distribution with intervals that are more than a single integer, as where the ages are grouped into four-year intervals, it will be necessary to let the X in the formula stand for the mid-point for each interval into which ages are grouped. This leads to small inaccuracies which you will not have if you code exact ages.

Limitations of the Mean with Asymmetrical Series

The mean -- or average -- is generally the best way to summarize a large series of numbers. It also has other statistical properties which make it even more valuable. However, it is not a good indicator of what is "typical" when the series of observations is markedly asymmetrical. This could happen if your program's clients consisted mostly of teenagers and young adults, with only a few persons of more advanced age. These few individuals can raise the mean considerably. Suppose all the females in your program were between 17 and 24, and that most of your males were also in this age range. However, if you happened to have a few males in their 30s, a mean would exaggerate the age differences between the sexes in your sample. When you have a few extreme cases, another descriptive statistic -- the median -- is more appropriate.

The Median

The median is simply the value of the middle case in your ordered series of numbers. It can also be used for "order statistics" such as ratings, where you are not certain of the precise interval between adjoining numbers. Suppose you had the following numbers (let them be the same series examined previously):

17, 19, 19, 21, 21, 22, 24, 27, 28, 29

The "middle" number in this series falls between 21 and 22. In this case you would locate the number halfway between these two numbers and designate the median as 21.5. Where there is an odd number in the series you would be able to select the middle case as the median without having to extrapolate. The same principle is involved if the data are in a frequency distribution, although the estimation of the particular value becomes a little more complicated. If this becomes necessary, you should consult a statistics text.

The advantage of the median is that it is not influenced by extreme deviations. In distributions where the cases are scattered symmetrically above and below the median, the two indices -- the mean and the median -- will be identical.

Rates

Another convenient way to summarize results is called a rate. This is applicable to those variables where neither means nor medians are appropriate. A rate -- of which a percentage is one example -- is simply a fraction or ratio. It represents the number of times an event occurs (being male, dropping out of the program), divided by the total relevant population. Because the denominator, the relevant population, will be larger than the numerator, the resulting figure is less than one, so that a decimal point would precede the rate.

Using a Constant Multiplier

To avoid decimalization, the results are multiplied by a constant, k. When k is 100, you have a percentage. However, if you are examining relatively rare events where the numerator is small, you may choose a k equal to 1,000 or any other convenient number. Death and crime rates are often reported as rates per 100,000. There should not be any confusion as long as the choice of k is made clear to the reader. The formula is as follows:

$\text{Rate} = \frac{f}{p}k$, where f = the subgroup of interest
p = the relevant total population
k = constant

Rates Reflecting Group Performance

Rates are easy to calculate and very useful. For example, in your follow-up, you could calculate the rate of employment at 12 months after discharge. However, you might also want to know not only how many are employed at this moment, but also the proportion of the time since terminating treatment that the average client has been employed.

In this case, the numerator (f) becomes the total number of months worked since discharge by all the clients in your follow-up sample, and the denominator (p) becomes the total number of months since discharge added up for all clients. If all respondents had the same length of time out -- for example, 12 months -- then p would be 12 times the number of individuals in your study.

The computation of both group and individual rates of employment over the interval since discharge is illustrated below. Individual rates would be useful if you plan correlational analysis, to show the degree of relationship between employment and some other variable, such as education, where you also have a score for each individual.

Client	No. Months Since Termination (p)	No. Months Employed (f)	Individual Rate $(\frac{f}{p})$
A	9	8	.888
B	17	12	.706
C	14	14	1.000
D	11	2	.182
E	23	16	.696
Total	74	52	3.472

Rate I = 3.472/5 = 0.6944 = .69 or 69%
Rate II = 52/74 = .702 = .70 or 70%

These data show for each client the number of months since termination and the number of months employed. It is usually sufficient to round off both numbers to the nearest whole month. However, more exact representation would not alter the procedure for finding a group rate outlined above.

Individual Rates vs. Group Rates

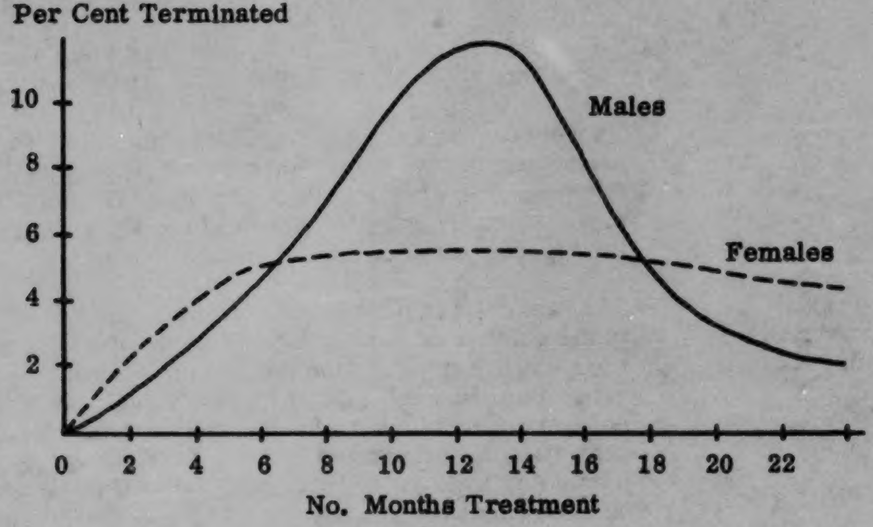
The last column presents the ratio of number of months employed to the number of months since leaving the program, giving a rate for each individual. One method of obtaining a rate for the entire group is to find the sum of the individual rates and divide by the number of individuals who entered into the calculations, in this case five, as illustrated by Rate I. However, you may have no particular interest in individual rates if the information will not be used for further analysis. The group rate can then be obtained by a second method -- simply dividing the number of months employed, 52, by the total number of months the sample has been terminated, 74. Rate II shows this procedure. If a few individuals had many more months since discharge than the rest, this second method allows the group's rate to be inordinately influenced by them.

Rate I treats each case as a single unit. Thus, respondent A with only nine months since termination has the same influence on the group rate as respondent E with 23 months. In the example given, the differences between the two methods are negligible. These two methods of obtaining a rate will usually yield very similar results unless there are very large differences among individuals in the number of months since termination.

VARIABILITY

In addition to measuring central tendency by calculating means, medians, and group rates, it is often useful to examine the distribution of the data across all individuals, or to compare the way it varies across groups. For example, it might be useful to compare the retention of male and female clients. The following graph presents a hypothetical set of data comparing the time of termination of male and female clients in a particular program. Because there are usually many more males than females in drug programs, the vertical or Y-axis is stated in terms of percentages rather than frequencies, to facilitate comparison.

Percentage Terminated Monthly for Male and Female Clients



The graph shows that both groups have varying lengths of time in treatment, but the males have a much more "modal" or typical pattern (of approximately 12 months of treatment) than do the females. Using only one index -- the mean -- would have obscured this difference, since the mean for men and women would both be about 12 months.

The Standard Deviation

When a graph is not used, it is possible to compute a variability index to summarize the shape the graph would have. This index is called the standard deviation.

Calculating the Standard Deviation

The formula is:

$$s = \sqrt{\frac{\Sigma (X-M)^2}{N - 1}}$$

where s = standard deviation
X = each particular observation
M = mean
Σ = sum
N = number of cases

To calculate the standard deviation, the mean is first subtracted from each observation. Where there is more dispersion there will be larger differences between the mean and the scores. The numbers thus obtained, as seen in the illustration below, are squared. Then the sum of the squared numbers is divided by the total number of observations minus one. (This subtraction is necessary for reasons too technical to explore here.) Finally, the square root of the number that is generated is calculated.

The calculations may be illustrated with the series of age data used in the previous section on the median as follows:

Calculation of Mean (M) and Standard Deviation (s)

X	$(X-M)$	$(X-M)^2$
17	-5.7	32.49
19	-3.7	13.69
19	-3.7	13.69
21	-1.7	2.89
21	-1.7	2.89
22	-0.7	0.49
24	1.3	1.69
27	4.3	18.49
28	5.3	28.09
29	6.3	39.69
ΣX=227	$\Sigma (X-M)$=0	$\Sigma (X-M)^2$=154.10

$$M = \frac{\Sigma X}{N} = \frac{227}{10} = 22.7$$

$$s = \sqrt{\frac{\Sigma (X-M)^2}{N-1}} = \sqrt{\frac{154.10}{10-1}}$$

$$= \sqrt{17.12} = 4.14$$

N = 10

The resulting standard deviation, 4.14, is an index of variability.

It is useful to examine the standard deviation whenever it is suspected that variability is very different among the groups being compared. As noted previously, two groups may have the same mean yet very different standard deviations. Giving both the mean and the standard deviation completely describes any normally distributed variable.

The standard deviation has other important properties when data are distributed in a normal curve and it serves as the basic statistic for many important tests used to determine statistical significance. A review of these properties of the standard deviation can be found in any statistics text.

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Chapter 10

THE EXAMINATION OF RELATIONSHIPS

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The previous chapter described several methods for organizing and presenting some of the descriptive information collected in a follow-up study. However, you generally want to know something about the relationships between pairs of measures (variables). These relationships may concern changes over time in the same variable - such as employment status on entering treatment and one year afterward. Or you may be interested in relationships between different variables - such as between the reasons for entering treatment and retention in the program.

THE DATA FOR ANALYSIS

Generally, you will have information only on the treated group and on those clients who withdrew or were discharged. This markedly influences the conclusions you are able to draw. It makes it difficult to infer that the treatment accounts for any changes that may be noted. Improvement, for instance, could be influenced by many things other than the treatment regimen. Only a comparison group of similar individuals who have not been in treatment would permit a clear inference that improvement means the program has been effective. On the other hand, if there has been no improvement, it is generally safe to say that the program is ineffective - unless there is reason to believe people could become worse without treatment.

However, important comparisons can be made even without an untreated or control group. Comparing the experiences and reactions of those who remain in treatment with the experiences of those who withdrew can help staff to innovate more successful program strategies. And you can learn what kinds of clients do and do not achieve program goals.

Types of Data

If you used the data collection procedures illustrated in Chapter 4, you will have collected data that reflect client status at three significant time points: before treatment, during treatment, and after treatment. The possibilities for combining and comparing information representing these different time periods are almost unlimited, but this chapter suggests a few comparisons in which you may be particularly interested. This chapter illustrates how to construct tables for the purpose of examining relationships, though it is also possible to answer many of the same questions using other techniques.

INTAKE—OUTCOME TABLES

No matter how well a program may select clients or manage its program, different clients will respond to the program differently. There are always those who decide to leave the program before staff thinks they should, and those whose behavior prompts the staff to discharge them. Those individuals who successfully complete treatment can be compared to those who entered with them but withdrew or were terminated.

Correlating Intake and Outcome Data

Suppose you want to know whether the reason for coming into the program has any bearing on how the client responds to treatment. To answer this question, you will have to examine reasons for entry simultaneously with information about which individuals complete the treatment cycle. ("Completion" will be defined according to how the program defines its treatment regimen.) Comparisons are made by cross-tabulating the information on these two types of data for each individual in the program.

How to Cross-Tabulate the Data

If carrying out the cross-tabulation by hand, you would first sort all cases into a few broad categories of reasons for entering the program. You would then sort each of these piles into those who completed treatment and those who terminated, and count the resulting piles. Where tabulating equipment is used, it carries out essentially the same procedure for you. The results might look like the following:

Reasons for Entering Program and Treatment Status

	Reasons for Entering Treatment		
Treatment Status	Environmental Influences	Client Needs	Program Qualities
Completed	35%	63%	[50%]*
Terminated	65%	37%	[50%]
	100%	100%	100%
	(23)	(40)	(10)

*Brackets indicate the percentages are based on only 10 cases.

This table makes clear that those clients who acknowledged some personal need are most likely to remain in the program. Where other persons or agencies influenced clients to enter the program, the proportion who completed the cycle appears to be substantially less, although certainly not negligible. Because the third reason, program qualities, was so rarely given, its effect cannot be studied. Note that its column in the table is bracketed to show the number provides too small a base for reliable percentages. This is a problem encountered frequently with small samples.

Establishing the Independent Variables

How do you establish which variables are independent and which dependent? The general rule is that something prior in time, such as reasons for entering treatment, is the putative "independent" variable. "Reasons" are an independent variable and "remaining in treatment" is dependent, because reasons for entering treatment can influence whether a person remains in treatment or leaves it, but the reverse is improbable. When the variables might occur in either order, the decision as to which to treat as independent or dependent is more a matter of your assumptions about the direction of causality.

Layout of the Table

The categories of the independent variable are usually located on the horizontal axis, as column headings, while the dependent variable is usually located on the vertical axis, as row labels. Percentages are then based on column totals. This conventional approach can be violated where other considerations arise. The most common reason for reversing their position is that the independent variable may be divided into too many categories to fit across the page. However, the reader must always know in which direction the percentages are computed and the number of cases on which percentages are based. This is done by including the totals (100 per cent) either at the bottom of the columns as in the previous table, or at the right of the rows when the independent variable is placed vertically, and by putting the number of cases in parentheses next to the 100 per cent or next to the category name.

Multiple Independent Variables

Many independent variables might influence a particular dependent variable. Among factors that you might consider are sex of the client, age, education, socioeconomic level, ethnic background, age at onset of drug use, whether there has been previous treatment, duration of addiction, criminal history and work experience. Each of these can be treated in a manner parallel to the way "Reasons for Entering Treatment" was used above, to see whether there appears to be a relationship between any of these and any dependent variable such as the completion of treatment.

Choosing Dependent Variables

Many dependent variables can also be studied--referring not only to the treatment experience but also to adjustment after treatment ends. One common dependent variable is staff's rating of improvement. For outpatient programs, regularity of attendance and percentage of urines positive for morphine may be useful. After treatment, self-reported drug use, arrests, and employment status may have interest. Any aspect of the program or life afterwards for which regular and consistent data have been collected can be an appropriate candidate for a dependent variable, so long as clients differ with respect to it. Choosing the particular variables to use will depend on the nature of the program, the areas that the staff view as problematic, and the importance of that aspect of the program.

MULTIVARIATE TABLES

Once you look at the many independent variables that may influence your dependent variables in bivariate tables (i.e., tables with one independent and one dependent variable), you are likely to discover that several independent variables were associated with a particular dependent variable. For example, you may find that employment rates are higher for males than females, for older clients than younger ones, for those who have more education, and for those who had a later age of onset of drug use. A full discussion of all the possible ways to proceed if you discover this to be true is beyond the scope of this manual. However, one option often brings clarity to the possibly confusing array of significant patterns that have been identified.

The first thing to do is to determine how the independent variables that you found related to your dependent variable are related to each other. For instance, you may note that older clients had a later age of onset of drug use than younger clients. The analytical problem, then, is to attempt to decide whether both chronological age of the client and age of onset are really important in predicting employment.

Or is only one making a difference? Only when both independent variables are examined simultaneously with the dependent variable can some insight be gained into which of these alternatives is correct.

Each respondent must now be classified simultaneously in three ways: by his chronological age, his age of onset, and whether he was employed at follow-up. Because you are substantially increasing the number of categories to be examined, it is often necessary to reduce the number of categories in each variable. How much collapsing of categories you have to do will depend on your sample size, the way the variables are distributed in your particular program, and the degree of the correlation or association between the variables. In the following tables, note that chronological age is dichotomized, while age of onset is grouped into three categories. You try to collapse the data so that you have reasonable numbers of cases in each category while losing as little information as possible.

Employment Status One Year After Treatment
By Current Age and Age at First Addiction

	Current Age					
	Age 21-30			Age 31 and Over		
	Age of Addiction Onset			Age of Addiction Onset		
Employment Status	Under 18	18-24	25+	Under 18	18-24	25+
Unemployed	38%	28%	20%	42%	29%	18%
Part-time employment	43%	44%	25%	42%	40%	24%
Full-time employment	19%	28%	55%	16%	31%	58%
	100%	100%	100%	100%	100%	100%
	(42)	(76)	(20)	(31)	(52)	(50)

This table has been constructed to make it possible to examine the relationship between age at onset of drug use and employment status separately for different age groups, and the effect of age groups separately for those with different ages of onset. This is what is meant by "controlling" on a variable--that is, holding one variable relatively constant while looking at the relationships between two other variables. The table shows for both age groups that the later the age of onset of addiction, the larger the proportion who are employed full-time at follow-up. Conversely, comparison of selected columns from each panel with the same age of onset -- such as those clients whose addiction occurred prior to their eighteenth birthday -- shows there is very little difference in employment by current age. Similar observations can be made for each pair of age-of-onset columns.

The table shows, then, that the significant factor relative to employment is age of onset, not current age. The only reason current age was originally found to relate to employment was that clients who are now older became addicted later in life.

If results had turned out differently, it might be concluded that the relationship between late onset and holding a job occurs only among older clients, that both factors influence employment, or that only current age has an important influence on employment. More details on this procedure may be found in Zeisel (1957) and in Rosenberg (1968).

EVALUATING PROGRAM OUTCOMES

Whom to Compare

The "pay dirt" for a follow-up study is to be able to find out whether those who have been treated have improved in significant areas of functioning. You may have obtained follow-up information on employment, income, family status, arrest records, self-reports on illegal behavior, self-reports on drug use, even urine analyses on the respondents at follow-up. The significant question -- one that turns out to be much more difficult than it may seem -- is whether there is [illegible] discernible increase in approved types of behavior or any decrease in those types of behavior that are considered deviant.

Factors other than treatment may be accounting for observed "improvements" or "declines," but you cannot tell whether this is so unless a comparison group is used. For example, if the labor market changes drastically, employment may be influenced positively or negatively irrespective of the quality of your employment training efforts. Similarly, changes in police enforcement policies might account for observed changes in arrest levels, rather than some real change in the criminal behavior of the clients. Only a "control" group can obviate alternative explanations for changes through time, and this is rarely available. Nevertheless, many useful things can be learned by follow-ups if these limitations are kept in mind.

One important limitation is that in the period from intake to termination you have lost many of your clients. In some programs most clients will not complete the treatment cycle. The overall trends of apparent improvement are often primarily a result of a process observed in all programs; namely, that those who withdraw or are terminated are, on the whole, more resistant to program efforts, and were generally more deviant to start with. Thus, at follow-up those who stay on have less crime, more employment, better family relations, and less drug abuse than those who leave, but this is not necessarily because they got more treatment. To avoid confusing differences in the kinds of clients who finish with effects of the program, you should take the following precautions: (1) contrast survivors with terminators as of the time they entered treatment, so you at least know how they differ beforehand; and (2) examine cohorts who entered together during the same time period and who completed treatment. Then any changes you observe cannot be due to the changing composition of the group under study or to historical changes in job opportunities or police practices.

Cohorts and Turnover

One potentially important outcome of treatment might be the employment status of clients who complete the prescribed course of treatment. Since clients who were working when they entered treatment will tend to continue to work, possible good effects of treatment on employment are probably limited to those not working at intake.

The following table shows how to report on the employment outcome of clients in a hypothetical program, taking work history before entry into account.

The table does not simply report that 50 per cent of the follow-up clients were employed at follow-up. Instead clients are first sorted according to their employment status at intake. Since this precedes subsequent employment, it is the independent variable and is located on the horizontal axis. Then on the vertical axis you tabulate the employment status at follow-up, which is the dependent variable. Since you may have cases who were not located or who gave no answers, you must account for these in your table. Usually, they should not be included in the base on which percentages are calculated. In the following table they are accounted for in a footnote. As in the previous sections the percentages are based on the sub-categories of the independent variable, employment at intake.

Employment Status at Intake and One Year After Completion of Treatment in Program XYZ

Employment One Year After Treatment	Employment Status at Intake		
	Employed	Unemployed	Total
Employed (104)	88%	34%	50%
Unemployed (102)	12%	66%	50%
Total: (206)*	100% (64)* 31%	100% (142)* 69%	100%

* Missing cases include 2 employed and 3 unemployed who refused to be interviewed.

Overall the table shows that 31 per cent of the clients who completed treatment had been employed at intake (64/206). At follow-up, this figure increased to 50 per cent (104/206). The table also shows that almost all of those who were employed at intake report employment later (88 per cent), while of those who were unemployed at intake, only about one-third (34 per cent) are gainfully employed after treatment. In other words, the slight loss of employment among those who were initially employed was more than compensated for by the employment of clients who were initially unemployed.

However, it cannot be safely concluded from this table that it was the program that improved the employment status of the group completing treatment. Other factors like maturation or an improvement in local economic conditions may have caused it. If a non-treatment comparison group failed to show a comparable increase in employment, then these alternative explanations might be ruled out. However, even without a comparison group, these findings may suggest that perhaps greater efforts in job training and placement should be directed toward those unemployed at intake. With all the constraints referred to in the design chapter, it appears that the status of the group may have been improved in the employment area as a result of treatment, but it should also be noted that there remains room for improvement.

The considerations illustrated in this section bear on efforts to show the effect of treatment on any outcome variable. The same procedures apply, and similar tables may be constructed, if the outcome variable has several categories instead of just two, or if rates calculated on the base of 1,000 or 10,000 were used instead of percentages. However, some important questions about criterion measures that arise in many follow-up studies have been skipped over. Some of these will be considered more fully in the next section.

MEASURING PROGRAM INDICATORS

It was noted earlier that some collapsing of categories is necessary in analysis to avoid small groups. This collapsing is vital for the independent variables, but is not so necessary for dependent variables, because they can be presented in both large and small categories in the same table. Too much collapsing loses essential information. In the previous table, for instance, employed could have been divided into:

	Employment Status at Intake	
Employment One Year After Treatment	Employed	Unemployed
Employed	88%	34%
Employed less than 6 months	12%	26%
Employed more than 6 months	76%	8%

This would have shown that not only are few clients who were unemployed at intake now employed, but even those now employed have been unemployed most of the time since leaving treatment. It has been assumed that all clients have had the same length of time since treatment.

Drug Use

In some earlier follow-up studies, drug use since discharge was often collapsed into "ever" and "never." The "ever" category combined clients who had used drugs a single time with those who were using drugs daily, on the supposition that even very occasional use would lead inevitably to compulsive use. Such a method tends to exaggerate the "failure" rate unduly: it is now clear that many occasional users do not necessarily move on to compulsive use, even if formerly addicted. It is best to subdivide the "ever" group into those who report occasional use, regular use but not daily, and daily users. Generally it is also necessary to do this for each type of drug separately, although you may also want to group all individuals who use any of the addictive drugs into one index, especially where your program deals with mixed addictions. Since some individuals may abandon the drugs that brought them to treatment and become dependent instead on alcohol, you may want to add heavy alcohol users to the clients viewed as having returned to drugs.

It is not proper to assume that anything less than regular use after treatment is evidence of program success. First, there may be under-reporting. Second, without an untreated control group it is not possible to draw the conclusion that diminished drug use is due to program participation, since other forces, such as aging, are also at work. Self-reported drug use, however, can be used to compare various sub-groups, such as the following: (1) those who completed the treatment cycle in contrast to those who failed to do so; (2) individuals who were exposed to different treatment modalities ; and (3) different ethnic, sex or age groupings of clients. In this way it is possible to learn what kind of clients need special help.

Employment

Aside from the problem of accuracy of employment reports, the major problem is how to cope with individuals for whom employment may not be a viable goal. This includes individuals who are unemployable because of physical or mental infirmities, women with children who choose to be homemakers, full-time students and those engaged in various forms of vocational training. One solution is to exclude the handicapped from the calculation, as was done with missing cases in the previous example, and then define homemaking, schooling, training, and working as all socially useful and aggregate them either in the form of a rate or a classification, on the assumption that all are equally valid options. Of course, the resulting classification is no longer simply employment but is better identified as "productive activity" or "socially useful activity." When this is done, the number of categories in the dependent variable should be expanded to specify: unemployed, full-time employment, part-time employment (e.g., less than 20 hours a week), homemaker, student, vocational training, unemployable. Or, where the sample is too small for such refinement, the categories can be limited to employed, unemployed, and other socially useful occupation, with an explanation of what groups went into the "other" category.

Crime

Where comparisons are made between pre-program and post-treatment arrest histories, it is important to use the same sources of information for both, so that self-reports are not being compared with police records. It is also desirable to have the pre-program index of criminal activity cover a several-year period in order to avoid inflating the level of prior criminal activity of clients. Since arrests are often the occasion for entering treatment, clients generally experience high arrest rates just preceding entry into treatment. It is questionable whether this unusual period should be compared with the period following treatment. Second, it is necessary to adjust for the fact that different clients will have different lengths of time involved. Those who enter treatment young will have had fewer years in which to get arrested. The procedure described in Chapter 9 for calculating individual employment rates can be used to construct individual arrest rates, which are then averaged. This equalizes the effects of different lengths of time among clients.

A second useful consideration, especially for maintenance programs, is to distinguish those charges directly related to drug use from those related to money-raising and other types of criminal activity. A program may obtain a spurious sense of accomplishment concerning crime reduction when drug arrests have declined, while other forms of criminal activity remain undiminished, or even increase.

Criminality tends to decline with age, and women and middle-class clients commit fewer crimes than others. Where a program serves a mixed age, sex, or socioeconomic group, it is often helpful to separate clients by age group, sex and other categories that reflect your particular client population when looking at posttreatment criminal activity.

STATISTICAL SIGNIFICANCE

The issue of statistical significance is a complex one. This section discusses the issues that should be considered when dealing with statistical significance and reviews one "model" that is appropriate for the form of analysis described in this manual.

Researchers are often interested in making the statement that the results observed in a particular table illustrate a true relationship. For example, a previous table showed that clients who enter treatment because of "Influences" terminated 65 per cent of the time, while those who came because of "Client Needs" terminated only 37 per cent of the time. The question is whether this pattern within the table reveals a relationship which is unlikely to have occurred in this sample simply by chance.

It is useful to understand just what is meant by "chance." For example, if you throw a pair of dice, there are 36 possible ways they can come down (each of the six faces of the first die combines with six faces of the second). Only six of these 36 give a score of seven. Assuming a perfectly balanced pair of dice, this means that the probability of getting a seven is one in six, or 17 per cent of the time. But if you tossed the pair of dice only ten times you might well find that you never got a seven at all (0 per cent), or you might get three sevens (30 per cent), neither very close to 17 per cent. However, if you tossed the dice 100 times the chances are that you would get sevens close to 17 per cent of the time. If you had the patience to toss the dice 10,000 times, you would very likely get something very close to 17 per cent of the tosses turning up as seven.

What does this mean about interpreting findings of follow-up studies? Where there are relatively few observations, then the "observed" values may scatter widely around the true value in the population from which the sample was drawn. Under the same conditions, the larger the sample of observations, the better your estimate of the true population value is likely to be. When two or more values are compared, both may deviate from the true population value, possibly widely if there are few cases or just a little if there are many cases. Unreliably large differences may appear -- that is, the observation of one variable may deviate in one direction, while observation of the second variable deviates in the opposite direction. Of course, there is also the reverse possibility, where differences appear more modest than they really are. It is precisely to take these random effects into account that statistical tests become important when drawing conclusions.

Statistical tests are simply ways in which knowledge about the size of the group being studied and the inherent variability of the data can be exploited to assign a probability that what you have observed could have happened by chance. The objective is to reduce the likelihood of asserting that a difference exists when in fact there is a reasonable probability that there is no true difference at all, and that the observed difference occurred only by chance. It is conventional to reduce this risk of error to five per cent or less, sometimes only one per cent, depending upon your judgment as to the consequences of asserting a difference when in fact you might be wrong. (See Chapter 3.) Most research analysts in programmatic research are satisfied with a five per cent level of significance.

Of course, even after testing for statistical significance, you will occasionally be wrong when you say a difference exists. If, for example, you achieve the five per cent level of significance, then you will be wrong about five per cent the time when you conclude there is a difference. The best way to reduce your uncertainty is by repeating the research. When successive replications continue to yield significant results, the risk of being wrong becomes very small (.05 x .05 = .0025 or .25 per cent instead of 5 per cent).

Testing Contingency Tables

To assess the significance of difference, the most common statistical test is the chi-squared (χ^2) test for contingency tables. The chi-squared test makes no assumptions about the kinds of measurements you have. It is appropriate for variables having only nominal classifications, like ethnic classification or sex. It can also be used for ranked variables like socioeconomic status or for variables with true absolute values, like age, although other tests that assume order or a true number scale might find significant results you will miss with chi-squared analysis. Different kinds of measures may be used in the same analysis. The chi-squared test compares your actual results (those you observe in your table) with the way the frequencies would have distributed themselves if the relationship had been produced by chance.

To illustrate the underlying logic of χ^2, a simple problem will be examined. When actually performing the calculations, there are more rapid methods that are reviewed in most standard texts. The objective here is only to illustrate the logic of hypothesis testing and, more particularly, the χ^2 test.

Suppose you suspected that female clients were more likely than males to have completed the treatment cycle in your program. The next table shows in the left panel the frequencies for terminated and retained clients by sex. Note that these are the actual numbers of clients and not percentages, because numbers are what you need for the χ^2 test.

Observed and Expected Frequencies for Sex Differences in Completion of Treatment

	OBSERVED			EXPECTED		
	Male	Female		Male	Female	
Terminated	60	20	80	57.12	22.88	80
Completed	90	40	130	92.88	37.12	130
Totals	150	60	210	150	60	210

The same procedure can be carried out for each of the four cells.

The table shows that 40 out of 60 women completed treatment (67 per cent), compared to 90 out of 150 men (60 per cent). This is a seven per cent difference. The question is whether a difference as small as that is significant, i.e., whether women do complete treatment more often than men, or whether this is simply within the range of differences likely to occur by chance.

In order to estimate how the cases would distribute themselves if sex and completion of treatment were completely independent, the matrix on the right side of the table has been constructed. It is based on the same principle as the previous calculation that the probability of getting a seven on a single throw of the dice is .17. It follows that if the probability of a single throw coming up seven is .17, the probability of getting two sevens in succession is (.17) x (.17) = .0289, or about three per cent. You are actually exploiting a basic law of probability theory which asserts that if two events are independent of each other, then the probability that they will occur jointly is the product of the probabilities of each event taken separately. In this example, probability of being male is 150 out of a total of 210 clients, or .714. The probability of being terminated is 80 out of 210, which equals .380. The product of .714 and .380 is .272. That is, if a client's probability of leaving treatment were independent of his/her sex, you would expect slightly over 27 per cent of all the cases to fall in the cell that intersects being male and terminated, or the top left cell in the table on the right panel. Taking 27 per cent of 210 cases, you get an expected number of 57.12. That is, if there was no relationship between sex and termination there would be only about 57 individuals in the upper left corner of the table. The right panel, then, shows how you would expect the cases to be distributed if sex and completion of treatment were independent of each other.

The Calculation of χ^2

What you have to do now is to assess the importance that can be attached to the differences between the observed and expected values in corresponding cells in the two panels of the table. The formula for doing so is:

$$\chi^2 = \Sigma \frac{(O-E)^2}{E}$$

Where: O = Observed frequencies

E = Expected frequencies

The formula simply informs you that for each cell you must subtract the corresponding expected values from the observed ones, square the value thus obtained, and divide it by the expected value. After doing this separately for each cell, you simply add up the obtained values, as indicated by the symbol Σ in the formula. Their sum is defined as the chi-square value.

It is usually easier to carry out these instructions with the layout illustrated in the following:

Calculation Format for Chi-Squared

(1) Observed	(2) Expected	(3) (0-E)	(4) $(0-E)^2$	(5) $(0-E)^2/E$
60	57.12	2.88	8.29	.145
20	22.88	-2.88	8.29	.362
90	92.88	-2.88	8.29	.083
40	37.12	2.88	8.29	.223
				$\Sigma = .819 = \chi^2$

Note that columns (3) and (4) all have the same number. This is a fact peculiar to 2 x 2 tables and would not be true for larger tables.

With contingency tables, the number of cells in a table influences the likelihood of random variability: the larger the number of cells, the greater opportunity there is for random effects. Therefore, the interpretation of χ^2 has to be tempered by this fact, and this is identified as "degrees of freedom." In contingency tables, the degrees of freedom are the number of rows minus one (r-1), multiplied by the number of columns minus one (c-1). In the above example there were two rows and two columns, and therefore (2-1) x (2-1) = 1. (This is merely another way of saying that if you know the totals and the value of one cell in a 2 x 2 table, you can calculate the other three cells. That is why the values in column 4 are identical.)

Testing the Statistical Significance of χ^2

In almost every statistics text there is a table of χ^2. Only a portion of such a table is given below.

Selected Values of χ^2

Degrees of Freedom	P=.20	P=.10	P=.05	P=.01
1	1.642	2.706	3.841	6.635
2	3.219	4.605	5.991	9.210
3	4.642	6.251	7.815	11.341

Note that in the first column are degrees of freedom and that in the column headings are various probability levels. Within the table are the χ^2 values that correspond to these probability levels. Note that for increasing degrees of freedom the values of χ^2 also increase for the identical probability levels, reflecting the phenomenon of more "random" effects as the number of categories increases. In each row, however, you can observe that as the values increase, the chance probability level declines: the higher the value of χ^2, the less the probability that differences observed were found by chance.

For the problem just calculated, it is apparent that the value obtained, $\chi^2 = .819$, is not as large as any value in the appropriate row for one degree of freedom. Thus, it is necessary to conclude that the differences found between the sexes could well have occurred by chance.

If the χ^2 value here had been at least as large, or larger, than 3.841, and if .05 had been chosen as an acceptable significance level, the conclusion would be that women are less likely to terminate.

The chi-squared test is only one of a great variety of possible ways of testing the significance of differences. A very large difference will be found significant by any test. You may want to ask for statistical consultation to decide how much credence to give to smaller differences. But you should not become a slave to significance testing and the magic of a .05 significance level. Important differences may exist but not show statistical significance because you have a small sample. On the other hand, if your sample is very large, a small and unimportant difference will be statistically significant even though it will be of no practical use. In the long run, it is not the statistical significance of a particular test, but whether your findings set an understandable pattern and survive replication in other samples, that determines whether your research has made important discoveries.

Chapter 11

REPORTING AND UTILIZING THE RESULTS OF A FOLLOW-UP STUDY

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The final stage in your research study will be to write a report -- summarizing the nature of the project, documenting the results, and spelling out what, if any, implications the results have for the administration of your program. Without a well-presented report, all the preceding efforts will be of limited value, because the project will not be in a form whereby others can review it and profit from its findings.

POTENTIAL AUDIENCES FOR THE REPORT

What you write about the study and how you write it are likely to be influenced by the type of readership you are assuming. Three of the more likely audiences for your report are discussed here.

Treatment Staff

Certainly the director and staff of the program itself will have an interest in the report and should receive copies. Presumably some or all will have taken a hand in conducting the study and perhaps also in writing the report. Nevertheless, it would be desirable for all of them to read the completed document and to benefit from whatever information and insights it may contain. In fact, it would be advisable to schedule a staff meeting to discuss the report, debate the validity of its conclusions, and consider its implications for the program. If the report is to be sent to outsiders, it may even be advisable to hold such a meeting before the final draft is completed so that insights gained at the meeting can be incorporated.

Sponsoring Agencies

If your follow-up study was conducted at the request of a sponsoring or funding agency, you may have no choice concerning whether or not to provide them with the report. On the other hand, if your study was self-initiated, you will have to decide whether distributing it to outside agencies is a desirable step. The fact that your program has been involved in systematic self-examination and is attempting to improve its procedures is likely to be seen as a plus.

If sponsoring agencies will receive the report, you probably will be particularly concerned about the quality of the writing, the rigor of the analysis and the description, and the overall appearance of the document. Also, if it is important to demonstrate program impact, you may decide to devote a more substantial proportion of your analysis and writing to that subject.

Other Audiences

It may be desirable to prepare your report for collateral agencies, such as those agencies who refer cases to you and to whom you refer cases for additional help. These might include legal aid or health and social agencies. In addition, you may want to send your report to individuals on city councils or state legislatures who have shown particular concern about drug abuse and who may be considering legislation related to it.

In general, results of follow-up studies are best used at a practical level, within the program itself, as a guide for the director and staff. Studies of single programs are not likely to produce findings which could be published in the professional literature, though some will. If yours is such a study, a separate article may be written for a particular journal using its required format and conventions. The article should not be written in lieu of a more comprehensive report, but rather should be based on some particular subset of the findings which will be of interest to a larger audience and which can be captured in a fairly brief article.

ORGANIZATION OF THE REPORT

Regardless to which of these audiences you address your report, it is likely that you will want to follow about the same general outline in writing it. While there obviously is no one "right way" to organize such a report, the topics outlined here should probably all be included in one form or another. Presentation in the following sequence should work well, unless you have some unusual requirements.

Abstract or Executive Summary

It is generally helpful to have a short statement at the beginning of the report which gives an overview of the study and notes any major findings. A one or two page abstract should be sufficient in most cases. If there are important consumers who are unlikely to read the full report -- funding agency executives, for example -- you may want to devote a few pages to an "executive summary" at the beginning. This summary will ordinarily follow the same outline as the whole report, but contain only a one or two sentence highlight from each section. It is useful to include page references for each heading to facilitate location of more detailed information in the main report. Although it is ordinarily placed in front of the report, the executive summary is the last section to be written.

Statement of Objectives

A good opening for the body of the report is a statement of the purposes for which the follow-up study was launched. These need not be belabored at great length, but it helps the reader as s/he proceeds through the remainder of the report to have in mind the research questions which are being addressed. If your objectives shifted during the course of the study it may be useful to note how and why they were revised.

STUDY DESIGN AND IMPLEMENTATION SECTION

The purpose of this section of your report is to document the way you went about designing and conducting the study. This section should be written in enough detail to allow independent judgments about whether the design was sound and whether the procedures were likely to lead to any systematic biases in the results. The reader does not need to know every name and number which passed through the process, but rather needs to understand the nature of the procedures and how they worked.

Staffing Responsibilities

This brief section should indicate who had overall responsibility for the direction of the project and (if they are different) who had major responsibility for the various stages such as interviewing and analysis. The role that those individuals normally play in the ordinary functioning of the agency should also be noted.

Study Design

The nature of the specific study design you chose to follow should be stated clearly, as well as your rationale for choosing it. A review of Chapter 3 might be useful in considering what points to include in this section.

Sampling Procedures

You should first make clear who was eligible for selection into the treatment group and the control or comparison group, and if possible how many eligible cases there were for each. Then you should indicate the method by which you selected a sample from those eligible, and how many cases you selected.

Data Collection Procedures

In this section it is helpful to list the data you tried to obtain with respect to each subject and how you went about getting it. This will include a description of the records you used and of the information you selected from them, as well as the methods used: (a) to locate respondents, (b) to set up interviews with them, and (c) to actually conduct the interviews.

This is also a good place to describe how the interviewers were chosen and trained, and to indicate some of the characteristics of the interviewing staff. For example, you might want to describe their composition in terms of sex, age, race, previous drug history, and counselor status. If counselors were used, you should indicate whether or not they interviewed their own former clients. Or, if interviewers were matched with clients in any other systematic manner, that also should be noted.

Tracking and Completion Rates

Since the interpretation of the results will and should be influenced by the adequacy of the obtained samples, it is very important to indicate what proportion (percentage) of each target group was finally located and what proportion (percentage) was actually interviewed. It is helpful to give these figures for important subsamples such as dropouts or older whites, so that the reader can judge how seriously to take your conclusions with respect to these sub-samples. If some sub-groups have been especially difficult to locate, those located may not be a representative sample of that sub-group, even when location rates for the sample as a whole are quite high.

Problems

It is advisable to mention any serious problems you may have encountered while conducting the study, so that the reader can have them in mind while digesting the results. It is also suggested that you indicate what, if any, impact the problems may have had on the final results. If you think there was any substantial impact, you will probably want to return to the topic again in your discussion of the results.

ANALYSES AND RESULTS SECTION

Exactly what subsections should be included in your discussion of the analyses (and the results to emerge from them) will depend on the type of research design you use and the major research questions you are trying to address. The several types of objectives of follow-up studies outlined in Chapter 1 suggest some sections you may want to consider, and are the basis for several of the subsections suggested below.

Characteristics of Clients Served

A good starting point for the section describing your program is to present some descriptive statistics on the client sample studied. Most of these data will have been gathered at intake.

It may also be useful to compare your sample with national or local statistics on the composition of other programs. Variables such as sex, age, ethnicity, drug history, and reasons for entering can be clearly displayed--probably in tables--to give some notion about the variety of client groups served by the program.

Program Completion and Associated Factors

Comparable descriptive information on any other control or comparison groups you have studied can be given in the same tables and/or in separate tables. Not only will that permit an examination of those who failed to start or complete treatment, but it can also facilitate an examination of the retention rates for clients having various characteristics.

Service Delivery

Insofar as you have included questions in your interviews concerning the quantity and types of various services received in the program, you may find that the nature of the services actually received by clients differed from what you intended. You may also want to show some descriptive tables for the whole sample and perhaps for some specific subgroups such as age or sex groups as well.

Client Reactions to the Program

Again, if you have included questions about how the clients see the program or feel about it, you will probably want to display the results in tables or graphs. Also, you may want to consider comparing different types of clients to see if they have (or had) different views.

Experiences and Behaviors After Treatment

Whether or not you have comparison or control groups, you will at least want to display as much information as you can on the natural history of your clients after they leave the program. This certainly should be done for the entire client sample and probably for important subgroups as well.

Group Comparisons in Outcome

Following the procedures outlined in Chapters 9 and 10, you also should present comparable data for whatever comparison or control groups you had in your research design, first emphasizing the natural history rather than attempting to explain what caused it.

Finally, you will have the more difficult task of trying to see what, if any, effect your treatment (or variations in treatment) had on the clients. Since the appropriate analysis methods are discussed at length in the preceding chapter, they will not be repeated here. However, it is probably worth mentioning once more that any interpretation of causality is likely to be quite tentative. Initial differences in the groups being compared should be taken into account to the fullest extent possible, and even after trying to take initial differences into account, there may remain some unidentified and unspecified differences between groups which help to explain differences in outcome.

DISCUSSION AND CONCLUSIONS SECTION

Presumably you will be discussing the nature of the findings and some of your interpretations of them as you write the analysis and results section. But you may want to develop a more general discussion of the entire project and its results. Be sure to include what you think the results mean and some notion of how confident you feel about them.

This is also an appropriate place to summarize the various results which have been discussed piecemeal in the previous section and, perhaps, to give some indication of their relative importance.

IMPLICATIONS FOR CHANGES IN THE PROGRAM

It is important to consider explicitly whether the results of the study have any implications for changing your program and, if so, what they are. There is a particular advantage to trying to commit these ideas to paper because you then will be forced to be more explicit about the logic. In addition, all interested parties will then have the same description in mind of what is being proposed, and why.

It may turn out that your study did not produce as clear a message as you might have wished about what changes would be advisable. But you should not feel that because a study does not produce a clear mandate for change that it was not worth doing. You will probably be much better informed about the likely benefits and costs of any change. Use your own judgment and that of the treatment staff about the importance and reliability of the findings and what they mean in deciding whether any changes should result. Letting staff and other interested persons review a draft of the report before writing its final section on implications for change may help to produce ideas that can then be incorporated into the final section.

APPENDIX A

CHOOSING THE UNIT OF ANALYSIS

The unit of analysis is the thing or person which serves as the focus of the study, the subject or subjects on which the data are to be gathered.

For the purposes of this manual it is assumed that the unit of analysis in most studies will be persons who have received drug treatment. This is by far the most conventional unit of analysis for the follow-up of drug treatment clients. However, it is not the only unit of analysis which could be considered.

Some researchers would argue that the effects a treatment program has on the broader community may be as important in the long run as its direct impact on the clients themselves. But it is generally not possible to study a program's effect on clients and on the community at the same time using the same research design. The clients and the community are entirely different units of analysis, and quite different types of studies are required to measure a program's impact on them. A couple of examples may illustrate the point.

The Crime Reduction Goal

One goal of most treatment programs is to reduce "drug-related" crime. That is, in addition to the goal of reducing client activity in the crimes of possessing or selling drugs, most programs also wish to reduce the general levels of such non-drug crimes as theft and prostitution, which are presumably increased by the presence of drug abusers in the community. In taking drug abusers off the street and involving them successfully in treatment, most programs have the collateral aim of lowering overall non-drug crime rates in their communities.

However, involving drug abusers in treatment may have little impact on community crime rates if the relationship between drug use and non-drug crime is weak or nonexistent, as some investigators have suggested (Silverman & Spruill, in press; Gould, 1974). On the other hand, it is also possible that a program may be unsuccessful in curbing drug abuse but successful in lowering crime rates. For example, methadone dispensed at a clinic may lower the demand for (and thus the price of) heroin being sold on the streets. In any case, success or failure with clients does not necessarily correlate with success in lowering overall community crime rates.

This example shows that the choice of one particular unit of analysis rather than another will restrict what can be said about the impact of a program. If the question is whether treatment lowers crime rates among clients, then clients are the appropriate unit of analysis. On the other hand, if the question is whether a drug treatment program has had an impact on overall community crime rates, then overall community crime rates are the appropriate unit of analysis. Assessing a program's impact on these different units of analysis would require two quite distinct types of studies, employing different designs, measurements, and analyses. This is why isolating the unit of analysis at the outset is essential.

The "Decriminalization" Goal

Another type of research question might also imply units of analysis other than clients. An example would be a study which seeks to learn whether a treatment program helps to "decriminalize" drug use. Simply stated, many treatment program personnel would argue that irrespective of whether or not they can "cure" drug abuse, it is a positive accomplishment if they can convince the community that drug abusers are ill, and therefore in need of treatment, rather than immoral, and thus in need of punishment.

To study decriminalization, units of analysis other than clients of the program would be needed. These might include, among others, dispositions of drug cases by the courts, accounts of the "drug problem" appearing in various communications media, or opinions held by the public. If causal links between changes in these various "indexes" of decriminalization and the presence of a drug treatment program in the community could be found, it could be argued that a certain kind of program "success" had been demonstrated.

REFERENCES

Gould, L. C. Crime and the Addict: Beyond Common Sense. Drugs and the Criminal Justice System. Edited by J. A. Inciardi; and C. D. Chambers. Beverly Hills: Sage Publications, 1974, pp. 54-75.

Silverman, L. P., and Spruill, N. L. Urban crime and the price of heroin. Journal of Urban Economics, in press.

APPENDIX B

TABLE OF RANDOM NUMBERS

10 09 73 25 33	76 52 01 35 86	34 67 35 48 76	80 95 90 91 17
37 54 20 48 05	64 89 47 42 96	24 80 52 40 37	20 63 61 04 02
08 42 26 89 53	19 64 50 93 03	23 20 90 25 60	15 95 33 47 64
99 01 90 25 29	09 37 67 07 15	38 31 13 11 65	88 67 67 43 97
12 80 79 99 70	80 15 73 61 47	64 03 23 66 53	98 95 11 68 77
39 29 27 49 45	66 06 57 47 17	34 07 27 68 50	36 69 73 61 70
00 82 29 16 65	31 06 01 08 05	45 57 18 24 06	35 30 34 26 14
35 08 03 36 06	85 26 97 76 02	02 05 16 56 92	68 66 57 48 18
04 43 62 76 59	63 57 33 21 35	05 32 54 70 48	90 55 35 75 48
12 17 17 68 33	73 79 64 57 53	03 52 96 47 78	35 80 83 42 82
65 81 33 98 85	11 19 92 91 70	98 52 01 77 67	14 90 56 86 07
86 79 90 74 39	23 40 30 97 32	11 80 50 54 31	39 80 82 77 32
73 05 38 52 47	18 62 38 85 79	83 45 29 96 34	06 28 89 80 83
28 46 82 87 09	83 49 12 56 24	88 68 54 02 00	86 50 75 84 01
60 93 52 03 44	35 27 38 84 35	99 59 46 73 48	87 51 76 49 69
22 10 94 05 58	60 97 09 34 33	50 50 07 39 98	65 48 11 76 74
50 72 56 82 48	29 40 52 42 01	52 77 56 78 51	80 12 43 56 35
13 74 67 00 78	18 47 54 06 10	68 71 17 78 17	74 35 09 98 17
36 76 66 79 51	90 36 47 64 93	29 60 91 10 62	69 91 62 68 03
91 82 60 89 28	93 78 56 13 68	23 47 83 41 13	09 89 32 05 05
17 46 85 09 50	58 04 77 69 74	73 03 95 71 86	40 21 81 65 44
17 72 70 80 15	45 31 82 23 74	21 11 57 82 53	14 38 55 37 63
77 40 27 72 14	43 23 60 02 10	45 52 16 42 37	96 28 60 26 55
66 25 22 91 48	36 93 68 72 03	76 62 11 39 90	94 40 05 64 18
14 22 56 85 14	46 42 75 67 88	96 29 77 88 22	54 38 21 45 98
91 49 91 45 23	68 47 92 76 86	46 16 28 35 54	94 75 08 99 23
80 33 69 45 98	26 94 03 68 58	70 29 73 41 35	53 14 03 33 40
44 10 48 19 49	85 15 74 79 54	32 97 93 65 75	57 60 04 08 81
12 55 07 37 42	11 10 00 20 40	12 86 07 46 97	96 64 48 94 39
63 60 64 93 29	16 50 53 44 84	40 21 95 25 63	43 65 17 70 82
37 08 92 00 48	61 19 69 04 46	26 45 74 77 74	51 92 43 37 29
42 05 08 23 41	15 47 44 52 66	95 27 07 99 53	59 36 78 38 48
22 22 20 64 13	94 55 72 85 73	67 89 75 43 87	54 62 24 44 31
28 70 72 58 15	42 48 11 62 13	97 34 40 87 21	16 86 84 87 67
07 20 73 17 90	23 52 37 83 17	73 20 88 98 37	68 93 59 14 16

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APPENDIX C

SAMPLE SIZES REQUIRED TO SHOW SIGNIFICANCE AT THE 95 PER CENT LEVEL (ONE-TAILED TEST) IN DIFFERENCES IN PROPORTIONS IN GROUPS OF EQUAL SIZE*

p_2	p_1 = 05%	10%	15%	20%	25%	30%	35%	40%	45%
10%	418	-	-	-	-	-	-	-	-
20%	84	195	791	-	-	-	-	-	-
30%	42	67	120	269	1064	-	-	-	-
40%	27	37	53	83	145	319	1237	-	-
50%	19	24	31	42	60	92	159	344	1311
60%	14	17	21	27	34	45	63	95	162
70%	11	13	15	18	22	27	35	45**	62
80%	9	10	11	13	16	18	22	27	33
90%	7	8	9	10	11	13	15	17	20

Note:

p_1 = the proportion or percentage of a group having a particular characteristic.

p_2 = the comparable proportion observed in another group. (p_1 should be assigned to the group having the higher proportion.)

---

* Adapted from Table A-3, Joseph L. Fleiss, Statistical Methods for Rates and Proportions. New York: Wiley Interscience, 1973.

** The circle is only applicable to the example in Chapter 3 and is not to be used for any other computation.

APPENDIX D

ESTIMATING PERCENTAGE DIFFERENCES BETWEEN PROPORTIONS

Upper and lower bounds may be calculated on the true difference between two rates from two populations, given an observed difference between two samples from those respective populations. For example, if you have a group-therapy sample which yields a 70 per cent success rate and an individual therapy sample which yields only a 40 per cent success rate, you have obviously observed a substantial difference in success. However, before making too much of the 30 per cent difference you may want to know how much confidence you can have that the size of that difference can be extrapolated to others who might go through your program. The following formula can be used to determine the upper and lower bounds of an interval within which you can guess the true population difference lies, and only expect to be wrong five per cent of the time. This is also called "the 95 per cent confidence interval."*

FORMULA

$$\text{upper limit:} \quad (p_1 - p_2) + \left[1.96 \sqrt{\frac{p_1\,(1-p_1)}{N_1} + \frac{p_2\,(1-p_2)}{N_2}} \right]$$

$$\text{lower limit:} \quad (p_1 - p_2) - \left[1.96 \sqrt{\frac{p_1\,(1-p_1)}{N_1} + \frac{p_2\,(1-p_2)}{N_2}} \right]$$

In the example used above, p_1 is 70 per cent, p_2 is 40 per cent and $(p_1 - p_2)$ is 30 per cent. If the number of cases in the group-therapy sample (N_1) was 50 and the number of cases in individual treatment (N_2) was 100, then the upper and lower limits of the difference in the portion of successes can be estimated as follows:

EXAMPLE

upper limit:

$$= (.70 - .40) + 1.96 \sqrt{\frac{.70\,(.30)}{50} + \frac{.40\,(.60)}{100}}$$

$$= .30 + 1.96 \sqrt{.0042 + .0024}$$

---

* These formulae yield a confidence level of 95 per cent. If a 90 per cent confidence interval were to be calculated, the constant term 1.96 in each formula would be replaced by the value 1.65. If an 80 per cent confidence level were judged acceptable, the term would be 1.28.

upper limit (continued):

$= .30 + .159$

$= .459$ or 45.9%

lower limit:

(Note that all computations are the same as for the upper limit, except that the second parenthetical term is subtracted from--rather than added to--the first term of $(p_1 - p_2)$.

$= .30 - .159$

$= .141$ or 14.1%

As a result of these calculations, we can state with 95 per cent certainty that the true difference in success rates lies between 14.1 per cent and 45.9 per cent. (As you can see from the formulae, increasing the sample size reduces the range of uncertainty.)

To take another, perhaps more realistic example, if we had observed "success rates" which were only 10 per cent apart (50 per cent versus 40 per cent), the lower and upper confidence limits would come out to be -6.9 per cent and 26.9 per cent. Note that, although group therapy was observed to have a higher success rate by ten percentage points, we cannot be certain (with 95 per cent confidence) that, in fact, group therapy is better. That is, our confidence interval (True p for group therapy-True p for individual therapy) includes a negative value, -6.9 per cent--which would mean that individual therapy might just have a better success rate, by as much as 6.9 per cent, if data from the entire population of clients were gathered.

APPENDIX E

INTERVIEW ITEMS

On the following pages are some sample questions. The great majority of them have actually been used in survey studies. Nevertheless, the admonition in Chapter 4 about the need for careful pretesting still applies. The items are grouped, as in Chapter 4, into Independent, Dependent, and Other Variables. In the left column is a brief phrase, indicating what the question is intended to measure. Many of the questions may not need to be asked at a follow-up interview if the answers were obtained in a baseline interview. An asterisk (*) indicates such an item.

If there are areas of interest to you which are not adequately covered, you can make up your own questions (and very carefully pretest them). Or, preferably, you may make use of two volumes published by the National Institute on Drug Abuse:

> "Research Monograph Series 2: Operational Definitions in Socio-Behavioral Drug Use Research 1975." Edited by Jack Elinson and David Nurco.
>
> "Research Issues 12: Drug Abuse Instrument Handbook." Edited by Alexis Nehemkis, Mary A. Macari, and Dan J. Lettieri

INDEPENDENT VARIABLES

DEMOGRAPHIC

Sex* (BY OBSERVATION)
- (1) Male
- (2) Female

Date of birth* What is your birth date?

____
(month, day, year)

Ethnic origin* (BY OBSERVATION) or, What is your race or ethnic background?
- (1) White (not of Hispanic origin)
- (2) Black (not of Hispanic origin)
- (3) American Indian
- (4) Alaskan native (Aleut, Eskimo Indian)
- (5) Asian or Pacific Islander
- (6) Hispanic-Mexican
- (7) Hispanic-Puerto Rican
- (8) Hispanic-Cuban
- (9) Other-Hispanic
- (10) Other (Specified: _______________)

Place of birth* Where were you born? ____________________________
(city, state, country)

Education What is the highest grade of school or year of college you completed?

GRADES OF SCHOOL

00 01 02 03 04 05 06 07 08 09 10 11 12

COLLEGE

13 14 15 16 17 +

What is the highest degree or diploma you have received?

(1) None
(2) Regular high school
(3) General Education Degree (GED)
(4) Associate Degree
(5) Bachelor Degree
(6) Post-Bachelor College Degree
(7) Other (Specify: ______________________)

Age Left School How old were you when you left school the first time for one year or longer?

AGE IN YEARS ________

FAMILY BACKGROUND

Father's Education* What is the highest level of schooling that your father completed?

(1) Grade school or less
(2) Some high school
(3) Completed high school (or equivalent)
(4) Some college
(5) Completed college
(6) Graduate or professional school after college
(7) Other (Specify: ______________)
(8) Don't know or does not apply

Mother's Education* What is the highest level of schooling that your mother completed?

(1) Grade school or less
(2) Some high school
(3) Completed high school (or equivalent)
(4) Some college
(5) Completed college
(6) Graduate or professional school after college
(7) Other (Specify: ______________)
(8) Don't know or does not apply

Father's Occupation*

What type of work did your father do mostly during the time when you were growing up?

(RECORD VERBATIM) ______________________________

____

(CODE FROM LIST BELOW) . . . . ____

Executives and heads of large companies Major professionals (doctor, lawyer, CPA)	1
Managers and heads of smaller businesses or organizations Other professionals (registered nurse (RN), certified social worker (MSW), teacher, accountant)	2
Administrative personnel (store manager) Owner of small businesses Professional artists (sculptor, musician)	3
Clerical and sales workers (bank teller, bookkeeper, stenographer, sales clerk, shipping clerk) Technicians (dental technician, draftsman) Self-employed or shop owner (newsstand, tailor shop) Para-professionals (trained counselor, other social worker)	4
Skilled manual workers (chef or head cook, licensed vocational nurse (LVN or LPN), electrician, machinist, painter, welder)	5
Semi-skilled workers (short order cook, hospital aide, apprentice, bus driver, guard, machine operator, taxi driver, waitress)	6
Unskilled workers (dishwasher, bus boy, garbage collector, janitor, laborer, peddler, shoe shiner, window cleaner)	7
Homemaker	8
Not able to classify	9

Mother's Occupation* What type of work did your mother do mostly during the time when you were growing up?

(RECORD VERBATIM) ____________________

(CODE FROM LIST ABOVE) . . . . ____

Religious Background* What was your religious upbringing?

(1) None
(2) Protestant
(3) Catholic
(4) Other Christian
(5) Jewish
(6) Muslim
(7) Other Non-Christian
(8) Other (Specify: ____________________)

Religiosity How important is religion in your life? Would you say not important, a little important, pretty important, or very important?

(1) Not important
(2) A little important
(3) Pretty important
(4) Very important

Age Left Home* How old were you when you first left home to live at some other location?

_________ YEARS OLD

Intact Home* Up until you were 16 years old, did you always live with both your real mother and real father?

(1) yes
(2) no

Parental Drug Use* How would you describe your father's use of alcohol during the time when you were growing up? (SHOW CARD A)+

(1) Never drank
(2) Light drinker
(3) Moderate drinker
(4) Heavy drinker
(5) Very heavy drinker

How would you describe your mother's use of alcohol during the time when you were growing up? (SHOW CARD A)

(1) Never drank
(2) Light drinker
(3) Moderate drinker
(4) Heavy drinker
(5) Very heavy drinker

+Show cards may be found at the end of the interview items.

Parental Criminality

Did either your mother or your father use heroin or other narcotics?
(1) Yes, father
(2) Yes, mother
(3) Yes, both
(4) No, neither

Has either your father or your mother ever been arrested?
(1) Yes, father
(2) Yes, mother
(3) Yes, both
(4) No, neither

Number of Siblings*

How many younger brothers and sisters do you have?

NUMBER: ________

How many older brothers and sisters do you have?

NUMBER: ________

Siblings' Drug Use*

Were any of your brothers and sisters heavy drinkers?
(1) Yes
(2) No

Did any of your brothers and sisters use heroin or other narcotics?
(1) Yes
(2) No

Siblings' Criminality

Have any of your brothers and sisters ever been arrested?
(1) Yes
(2) No

MILITARY EXPERIENCE*

Have you ever been a member of the armed forces?
(1) Yes
(2) No ---SKIP REMAINING MILITARY QUESTIONS

Did you serve on active duty? (don't count Reserves, National Guard, or ROTC)
(1) Yes (ASK A.)
(2) No

A. When? From ____________ (month, year) to ______________________ (month, year--expected date if still on active duty)

Did you serve in Vietnam after 1969?
(1) Yes
(2) No

TREATMENT

Before Entry*

"Later I'll ask you some questions about the treatment program you were in at _______________, which ended in (termination date, month and year) _____________. But first,... "

Had you ever seen a doctor or been to a clinic or treatment center because of using narcotics before coming to this program?

(1) Yes . . . ASK A
(2) No . . . . SKIP TO QUESTION _____

IF YES:

A. How many times had you been in treatment for narcotics, altogether, before coming to this program?

NUMBER OF TIMES __________

After Release

Have you seen a doctor or been in a treatment center because of using narcotics since leaving the program?

(1) Yes . . . ASK A
(2) No . . . . SKIP TO QUESTION _____

IF YES:

A. Approximately how long have you been in treatment since leaving _______________?

NUMBER OF MONTHS ________

How many times have you been in treatment for narcotics, altogether, since leaving this program?

NUMBER OF TIMES __________

Are you still in a treatment program?

(1) Yes
(2) No

ASK EACH QUESTION FOR EACH EPISODE OF TREATMENT BEFORE ENTERING PROGRAM

		EPISODE		
		#1	#2	#3
Dates*	When did you enter treatment? (MONTH, YEAR)			
	When did you leave treatment? (MONTH, YEAR)			
Reason Left*	Why did you leave treatment? (1) completed treatment (2) transferred to another program (3) discharged for noncompliance with rules (4) left before completing program (5) incarcerated			
Medications*	What medications were prescribed?[+] (SHOW CARD B) (0) none (1) Methadone (2) LAAM, long-acting methadone (3) Propoxyphene-N, Darvon-N (4) Naloxone (5) Cyclazocine (6) Disulfiram, Antabuse (7) Naltrexone or other antagonist (8) Valium, Librium (9) Other tranquilizers (10) Other (Specify: ____________)			
Treatment Types*	What type(s) of treatment did you have? (SHOW CARD C) (1) Methadone maintenance (3 weeks or more) (2) Detoxification (includes methadone less than 3 weeks) (3) Counseling (4) Psychotherapy (5) OT or RT (6) Group Therapy (7) Narcotic antagonist (8) Job training			

[+] Remember to assign a separate column for coding for each category, since more than one answer is possible.

DEPENDENT VARIABLES

EMPLOYMENT

During the last eight weeks, what were you doing most of the time--which one of the things listed on this card? (SHOW CARD D)

(1) Working full-time
(2) Working part-time
(3) With a job, but not at work because of temporary illness, vacation, strike, or temporary layoff
(4) Unemployed, permanently laid off
(5) Retired
(6) In school
(7) Homemaking
(8) Other (Specify: ______________)

During the past eight weeks, about how many days did you work full-time (that is, seven hours or more) on legitimate jobs? RECORD NUMBER OF DAYS ACTUALLY WORKED. REMEMBER, THERE ARE 40 WEEKDAYS IN THE TIME PERIOD, 8 SATURDAYS AND 8 SUNDAYS.

NUMBER OF DAYS ___________

During the past eight weeks, about how many additional days did you work part-time on legitimate jobs?

NUMBER OF DAYS ___________

Job Type

IF ANY FULL-TIME OR PART-TIME WORK, ASK Q'S

OTHERS, SKIP TO _________

9. A. What kind of work did you usually do during the past eight weeks? That is, what is your job called? (What are some of your main duties?) RECORD VERBATIM.

B. What kind of a place do you work for? What do they make or do? RECORD VERBATIM.

C. CODE RESPONDENT'S JOB; THEN GO TO D BELOW.

Code _____

Executives and heads of large companies Major professionals (doctor, lawyer, CPA)	 1
Managers and heads of smaller businesses or organizations Other professionals (registered nurse (RN), certified social worker (MSW), teacher, accountant)	 2
Administrative personnel (store manager) Owner of small businesses Professional artists (sculptor, musician)	 3

Code _____

Clerical and sales workers (bank teller, bookkeeper, stenographer, sales clerk, shipping clerk)
Technicians (dental technician, draftsman)
Self-employed or shop owner (newsstand, tailor shop)
Para-professionals (trained counselor, other social worker) 4

Skilled manual workers (chef or head cook, licensed vocational nurse (LVN or LPN), electrician, machinist, painter, welder) 5

Semi-skilled workers (short order cook, hospital aide, apprentice, bus driver, guard, machine operator, taxi driver, waitress) 6

Unskilled workers (dishwasher, bus boy, garbage collector, janitor, laborer, peddler, shoe shiner, window cleaner) 7

Not able to classify 8

D. When did you start that job? ☐☐☐☐

Number of Employers How many different employers (including current employer) have you worked for in the last eight weeks, on legitimate jobs?

NUMBER OF EMPLOYERS ___________

Amount of Pay How much money did you make altogether--that is, take home pay--from (that/those) job(s) during the past eight weeks?

DOLLARS ___________

Sources of Support Please look at this card and tell me all of the things listed on it which have been legitimate sources of money or support to you during the past eight weeks. CODE ALL MENTIONED. IF MORE THAN ONE, ASK A. (SHOW CARD E)

(1) Job
(2) Unemployment compensation
(3) Disability or workmen's compensation
(4) Public assistance or welfare
(5) Treatment program
(6) Husband or wife
(7) Other family
(8) Boyfriend or girlfriend
(9) Other friends
(10) Other (Specify: ___________)

IF MORE THAN ONE:

Major Source A. Which one did you get the most money or support from?

CODE ____________

Illegal Source Have you had any illegal sources of making money during the past eight weeks? (I don't need to know any details.)

(1) Yes . . . ASK A
(2) No . . . . SKIP TO QUESTION ____

IF YES:

A. Has that been a major source of money or support to you during the past eight weeks?

(1) Yes
(2) No

Schooling During the past eight weeks, have you attended school at all?

(1) Yes
(2) No

Training During the past eight weeks, have you had any vocational or job training (other than that you just mentioned)?

(1) Yes
(2) No

TOBACCO AND ALCOHOL USE

Tobacco Use Do you smoke cigarettes?

(1) Yes . . . ASK A
(2) No

IF YES:

A. About how many cigarettes do you smoke each day?

(1) Only smoke occasionally
(2) Less than one pack
(3) About one pack
(4) Between one and two packs
(5) About two packs
(6) More than two packs
(7) Other (Specify: ____________)

Alcohol Treatment Are you currently in any kind of alcohol treatment program?

(1) Yes
(2) No

Alcohol Problems

	A. Have you ever had (READ EACH ITEM a-d) because of your drinking?		B. ASK B FOR EACH YES IN A: Was that more than 2 months ago or during the past 2 months, or both?		
	Yes	No	More than 2 months ago	During past 2 months	Both
a. Medical problems	1	2	1	2	3
b. Problems on a job	1	2	1	2	3
c. An Arrest	1	2	1	2	3
d. Family trouble	1	2	1	2	3

Alcohol Use

About how often have you been drinking during the last two months? (SHOW CARD F)

(1) Every day
(2) Almost every day
(3) 3 to 5 times a week
(4) Once or twice a week
(5) Once or twice a month
(6) Not at all

Occasions Heavy Use

In the last two months, on how many occasions have you had five or more drinks in a row?

NUMBER OF OCCASIONS ____________

Drug Use

Now I'd like to ask a few questions about drug use. During the past two months, about how often have you used (READ EACH ITEM 1-9) --would you say five to seven days a week, one to four days a week, less than once a week, or not at all? (SHOW (a-d) on CARD G)

	5-7 days per week	1-4 days per week	Less than once a week (occasionally)	Not at all	Other (Specify)
(1) Marihuana (CARD G-1)	1	2	3	4	5
(2) Hallucinogens (CARD G-2)	1	2	3	4	5
(3) Amphetamines (uppers) not prescribed (CARD G-3)	1	2	3	4	5
(4) Cocaine (CARD G-4)	1	2	3	4	5

	5-7 days per week	1-4 days per week	Less than once a week (occasionally)	Not at all	Other (Specify)
(5) Tranquilizers (CARD G-5)	1	2	3	4	5
(6) Barbiturates (downers) not prescribed (CARD G-6)	1	2	3	4	5
(7) Heroin (CARD G-7)	1	2	3	4	5
(8) Methadone not prescribed (CARD G-8)	1	2	3	4	5
(9) Other opiates not prescribed (CARD G-9)	1	2	3	4	5
(10) Other drugs not prescribed (Specify)	1	2	3	4	5

CRIMINAL ACTIVITY

Arrests

Have the police booked you on a charge or case in the past two months?

(1) Yes . . . . (ASK A-C)
(2) No

IF YES:

A. How many times were you booked in the past two months?

NUMBER OF TIMES ____________

Charges

B. ASK FOR EACH TIME MENTIONED IN A: What were the charges (each time)? RECORD BRIEFLY.

FIRST TIME: ____

SECOND TIME: ____

THIRD TIME: ____

C. REVIEW CHARGES MENTIONED IN B, AND RECORD NEXT TO CATEGORIES BELOW. So you were charged (Number) times with (CRIME), does that seem correct? CONTINUE UNTIL ALL CHARGES ARE CODED BELOW.

	Number of times charged
Crimes against persons--e.g., assault, rape, battery, homicide, manslaughter.	______
Crimes of profit--e.g., stealing, armed robbery, robbery, burglary, forgery, theft, retaining money under false pretenses.	______
Drug violations--e.g., dealing, possession	______
Prostitution, pimping or soliciting	______
Other (including minor offenses)	______

Police Contact

During the past two months have you been picked up by the police (taken to the station), but not booked on a charge or case?

(1) Yes . . . . (ASK A)
(2) No

A. IF YES: How many times? NUMBER OF TIMES ________

Self-Reports

Now, without telling me any of the details, have you done any of the things listed on this card during the last 2 months for which you were not charged by the police? Let me start with the first category.

Have you done any of the things in category (I) in the last 2 months? Just tell me yes or no. (SHOW CARD H) (REPEAT QUESTION FOR CATEGORY II, III, etc.)

			Yes	No
(a)	I	Shoplifting, pickpocketing, burglary	1	2
(b)	II	Purse-snatching, armed robbery, forgery, auto theft, blackmail, or extortion	1	2
(c)	III	Arson, vandalism, or other damaging of property	1	2

			Yes	No
(d)	IV	Assault, battery, mugging, rape	1	2
(e)	V	Soliciting, pimping, prostitution	1	2
(f)	VI	Dealing in drugs, forging prescriptions, stealing drugs	1	2

FOR FOLLOW-UP CLIENTS

Treatment Evaluation--General

Please think back to the treatment program you were in at ______________________ which ended (TERM. DATE, MONTH & YEAR): ______________________.

A. At that time was (   ) very easy for you to get to, fairly easy to get to, or hard to get to?
   (1) Very easy
   (2) Fairly easy
   (3) Hard

B. Did most of the treatment staff you knew there perform their jobs very well, pretty well, or not well, as far as you could tell?
   (1) Very well
   (2) Pretty well
   (3) Not well
   (4) Don't know (VOLUNTEERED)

C. Did you gain a lot of understanding of your drug problems, some understanding, or not very much understanding, as a result of treatment at that time?
   (1) A lot of understanding
   (2) Some
   (3) Not very much
   (4) Don't know (VOLUNTEERED)

D. Did the staff there try to help you with other kinds of problems in your life or did they work only on drug problems?
   (1) Other kinds of problems too
   (2) Only drug problems
   (3) Don't know (VOLUNTEERED)

Did you feel that any of the staff there cared about you as an individual and how your life turned out?
   (1) Yes
   (2) No
   (3) Don't Know (VOLUNTEERED)

How much individual treatment did you get in that program--did you get a lot, some, a little, or none?

(1) A lot
(2) Some
(3) A little
(4) None
(5) Don't know (VOLUNTEERED)

Do you wish you could have had more individual attention?

(1) Yes
(2) No
(3) Don't Know (VOLUNTEERED)

Were you very satisfied with the drug treatment you received there, somewhat satisfied, or not at all satisfied?

(1) Very satisfied
(2) Somewhat satisfied
(3) Not at all satisfied
(4) Don't know (VOLUNTEERED)

(No matter how you felt about your own experience there) was the program at that time good enough to recommend it to others?

(1) Yes
(2) No
(3) Don't Know (VOLUNTEERED)

As far as you know, does the program have a good reputation, fair reputation, or poor reputation among drug users in this area?

(1) Good
(2) Fair
(3) Poor
(4) Don't Know (VOLUNTEERED)

Was there anything you particularly liked about the drug treatment program?

(1) Yes . . . ASK A
(2) No

A. IF YES: What did you particularly like? (RECORD VERBATIM)

____

Anything else? ________________________________

Was there anything you particularly disliked about the program?

(1) Yes . . . ASK A
(2) No

A. IF YES: What did you particularly dislike? (RECORD VERBATIM)

____

Anything else? ________________________________

Length Drug-Free

How long was it after you left the treatment program before you took the drug (OR: one of the drugs) you had been treated for again?

(1) Same day
(2) 2 days - 6 days
(3) 1 week - 4 weeks
(4) 1 month - 3 months
(5) more than 3 months
(6) Never (skip to question _____)

What drug was that?

(list drug code)

Treatment Evaluation--Specific

Now I have some more specific questions about the program. I'm going to read you some of the complaints clients have had about various drug programs. These don't necessarily apply to the program you were in at ____________________ (SPONSORING AGENCY). However, I would like to know if any of these do apply to your experience at ______________________ (SPONSORING AGENCY).

(CIRCLE APPROPRIATE CODE FOR EACH COMPLAINT. AFTER EACH YES, ASK:) "Was that only a small or occasional problem or was it a big problem there?"

"Okay, here is the first complaint:	Not a Problem	Small or Some Problem	Big Problem
(1) Too little contact with doctors--Did that apply to your experience at ____________________ (SPONSORING AGENCY)?	1	2	3
(2) Foreign doctors who couldn't understand what you said	1	2	3
(3) Too much medication given out	1	2	3
(4) Too little medication given	1	2	3
(5) Too little privacy--you couldn't get away from other clients	1	2	3
(6) Too boring, not enough to do	1	2	3
(7) Too many unnecessary rules	1	2	3
(8) Not treated with respect by staff	1	2	3

	Not a Problem	Small or Some Problem	Big Problem
(9) Not enough staff	1	2	3
(10) Illicit drugs available	1	2	3
(11) Clients were kept in treatment too long	1	2	3
(12) Clients were made to leave treatment too early	1	2	3
(13) People were put on methadone maintenance who didn't really need it	1	2	3
(14) Not enough help getting job	1	2	3
(15) Not enough opportunity for job training	1	2	3
(16) Too little opportunity for group therapy	1	2	3
(17) Too little opportunity for individual therapy or counseling	1	2	3

Perceived Helpfulness of Treatment

"Being in a drug problem can help clients in a lot of different ways. Did being in the program at ______________________ (SPONSORING AGENCY) (from ______________ (MONTH, YEAR) to ______________ (MONTH, YEAR) help you in any of the following ways?"

(IF THE ANSWER IS NO TO ANY OF THESE, ASK: "Did you need that kind of help when you were there?")

	Yes	No, but Not Needed	No, and Needed
(1) Did the program reduce your use of the drug you were being treated for?	1	2	3
(2) Did the program get you off the drug that you were being treated for?	1	2	3

	Yes	No, but Not Needed	No, and Needed
(3) Did the program help you get off drugs other than your main drug of abuse?	1	2	3
(4) Did the program help you stay out of jail?	1	2	3
(5) Did the program help you get a job?	1	2	3
(6) Did the program help your state of mind, make you feel better, less depressed, etc.?	1	2	3
(7) Did being in the program get you help with a physical health problem?	1	2	3
(8) Did the program help you reduce your drinking?	1	2	3
(9) Did the program help you reduce your smoking?	1	2	3
(10) Did the program give you an opportunity to make new friends you still have?	1	2	3
(11) Did the program give you an opportunity to get more education or training than you would have otherwise?	1	2	3
(12) Did the program help you learn to get along with other people better?	1	2	3

OTHER VARIABLES

Living Place

Where have you lived most of the time for the past two months--which of the places listed on the card? (SHOW CARD I)

(1) Apartment or single family house . . . . (ASK A)
(2) Hotel . . . . . . (ASK A)
(3) Rooming or boarding house . . . . . . (ASK A)
(4) Hospital . . . . . . (ASK A)
(5) Therapeutic community (not hospital). . . . . . (ASK A)
(6) Jail or prison . . . . . . (ASK A)
(7) Other (SPECIFY & ASK A) ______________________________
____

(8) No regular place

Stability of Residence

A. IF CODES 1-7: How many places (except jail, prison or therapeutic community) have you lived in during the past two months?

NUMBER OF RESIDENCIES ____________________

Living With

Who have you been living with most of the time for the past two months? (PROBE: Anyone else?) CODE ALL THAT APPLY. (SHOW CARD J)

(1) Husband/wife
(2) Boy friend/girl friend
(3) Other friends
(4) Parents
(5) Other relatives
(6) Alone
(7) Other (Specify: ____________________)

Marital Status

Are you currently married, separated, divorced, widowed, or haven't you ever been married?

(1) Married (or living as married) . . . . . ASK A & B
(2) Separated . . . . ASK A & B
(3) Divorced. ASK A
(4) Widowed . . . . ASK A
(5) Other (Specify: ______________) ASK A
(6) Single, never married

A. IF EVER MARRIED, CODES 1-5: Altogether, how many times have you been married--legally, common law, or living as married?

NUMBER OF TIMES __________

B. IF CODES 1-2: How satisfied are you with your marriage--would you say very satisfied, somewhat satisfied, or not at all satisfied?
(1) Very satisfied
(2) Somewhat satisfied
(3) Not at all satisfied

Number of Dependents

How many persons are dependent, partially or totally, on you for financial support at the present time (not including yourself)?

NUMBER OF PERSONS ____________

Urine Sample

That's about all the questions we have. The last thing we need is a urine sample. It will be sent to a lab, where it will be analyzed for drugs. Your name will not be on it (SHOW ADDRESSED CONTAINER), and it will never be identified with you in any way.

CODE WHETHER GAVE SAMPLE OR NOT:
(1) Gave sample
(2) Unable to give
(3) Refused (Specify reason: ________________________)

Urine Results

ASK WHETHER OR NOT GAVE SAMPLE.

The urine will (would) be checked for all of the drugs on this card. The drugs usually show up on the tests if a person has taken any within the last three or four days. And, of course, they show up whether they were prescribed by a doctor or bought on the street. (SHOW CARD K)

Do you think any of these might show (have shown) up in your urine?
(1) Yes . . . . . . ASK A
(2) No

A. IF YES: Which ones? READ AND CODE EACH ITEM. THEN ASK (1) AND (2) FOR EACH "YES" BEFORE GOING TO NEXT.

ASK FOR EACH "YES" EXCEPT HEROIN: Are you using (DRUG) under a doctor's direction for medical treatment, or on your own?

	Yes	No	Doctor's Direction	On Own
Heroin?	1	2	XXX	XXX
Methadone?	1	2	1	2
Codeine?	1	2	1	2
Other Narcotics?	1	2	1	2
Uppers?	1	2	1	2
Downers?	1	2	1	2

Other Medicines

ASK IF GAVE SAMPLE:

Have you taken any (other) medicines at all in the last two days? I'm asking because certain medicines can be confused by the laboratory with the drugs they are testing for.

(1) Yes (Specify: ______________________________)
(2) No

SHOW CARDS

Cards labeled A, B, C, D, etc. can be printed and arranged in alphabetical sequence on a binder ring for easy reference by both the interviewer and the respondent. Anytime a show card is used with a question, the interviewer should turn to the appropriate card on the ring and hand it to the respondent. If the respondent cannot read, the interviewer can simply read contents out loud.

CARD A

(1) Never drank
(2) Light drinker
(3) Moderate drinker
(4) Heavy drinker
(5) Very heavy drinker

CARD B

(0) None
(1) Methadone
(2) LAAM, long-acting methadone
(3) Propoxyphene-N, Darvon-N
(4) Naloxene
(5) Cyclazocine
(6) Disulfiram, antabuse
(7) Other antagonist
(8) Valium, librium
(9) Other tranquilizers (meprobamates, haldol, thorazine, stelazine, mellaril, miltown, etc.)
(10) Other (Specified: ______________)

CARD C

(1) Methadone maintenance (3 weeks or more on methadone)
(2) Detoxification (includes methadone less than 3 weeks)
(3) Counseling
(4) Psychotherapy
(5) OT or RT
(6) Group therapy
(7) Narcotic antagonist
(8) Job training

CARD D

(1) Working full time
(2) Working part time
(3) With a job, but not at work because of temporary illness, vacation, strike, temporary layoff
(4) Unemployed, permanently laid off
(5) Retired
(6) In school
(7) Homemaking
(8) Other (SPECIFY)

CARD E

(1) Job
(2) Unemployment compensation
(3) Disability or workmen's compensation
(4) Public assistance or welfare
(5) Treatment program
(6) Husband or wife
(7) Other family
(8) Boy friend or girl friend
(9) Other friends
(0) Other (SPECIFY)

CARD F

(1) Every day
(2) Almost every day
(3) 3 to 5 times a week
(4) Once or twice a week
(5) Once or twice a month

CARD G

(a) 5 to 7 days per week
(b) 1 to 4 days per week
(c) Less than once a week (Occasionally)
(d) Not at all

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(1) Marihuana (or hashish)*
(2) Hallucinogens (LSD, mescaline, peyote, psilocybin, DMT)
(3) Amphetamines ("uppers," dexedrine, methedrine, dextroamphetamine, ritalin)
(4) Cocaine
(5) Tranquilizers (valium, librium, meprobamates, haldol, thorazine, stelazine, mellaril, miltown)
(6) Barbiturates and other sedatives ("downers," barbital, seconal, quaalude, methaqualone)
(7) Heroin
(8) Methadone
(9) Other opiates (opium, morphine, codeine, demerol, dilaudid, paregoric, talwin, laudanum)

CARD H

I. Shoplifting, pickpocketing, burglary
II. Purse-snatching, armed robbery, forgery, auto theft, blackmail, extortion
III. Arson, vandalism, or other damaging of property
IV. Assault, battery, mugging, rape
V. Soliciting, pimping, prostitution
VI. Dealing in drugs, forging prescriptions, stealing drugs

CARD I

(1) Apartment or single family house
(2) Hotel
(3) Rooming or boarding house
(4) Hospital
(5) Therapeutic community (not hospital)
(6) Jail or prison
(7) Other (SPECIFY)
(8) No regular place

CARD J

(1) Husband/wife
(2) Boy friend/girl friend
(3) Other friends
(4) Alone
(5) Other (SPECIFY)

*Actually, nine different cards should be used here, one for each drug, with the description of the drug at the top of the card.

CARD K

a. Heroin
b. Methadone
c. Codeine
d. Other narcotics (opium, morphine, codeine, demerol, dilaudid, paregoric, talwin, laudanum)
e. Uppers (amphetamines, cocaine, dexedrine, methedrine, dextro-amphetamine, ritalin)
f. Tranquilizers (valium, librium, meprobamates, haldol, thorazine, stelazine, mellaril, miltown)
g. Downers (barbiturates, barbital, seconal, quaalude, methaqualone)

APPENDIX F

SAMPLE LETTER OF INTRODUCTION FOR INTERVIEWERS

Dear Respondent:

I have written this letter to introduce you to an interviewer who will have an identification card from the __X__ Research Center. The Center is assisting us in a follow-up study. This is a continuation of the evaluation that was done while you were in treatment here. The interviewer will talk with you about how your life has been since then.

The purpose of this study is to learn how different types of treatment help different types of people with their problems. You are one of 200 people who have been randomly selected for interview.

You will be paid a fee of __X__ dollars for your time and consideration.

Your participation is purely voluntary and you have the right to refuse to be interviewed without fear of any consequences for doing so. However, I hope that you will cooperate, as this information is greatly needed in order to find out how well various treatments work.

Because of the laws of confidentiality, none of this information you give can be released to anyone, including us, without your written permission.

I am personally convinced of the importance of this project. If you wish to discuss this project with me or our staff, please feel free to do so.

Sincerely yours,

Director
__X__ Treatment Center

APPENDIX G

SELECTED CASE STUDIES: LOCATING RESEARCH SUBJECTS *

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Case No. 3:	Reluctance to Cooperate	179

FEAR OF ARREST

Some respondents live in constant fear of arrest because of criminal activity apart from drug law violation. Their determined hiding from police considerably complicates the study staff's efforts to locate them; moreover, their fugitive status heightens their suspicion of the study's "real" motive, and increases their reluctance to be interviewed. These obstacles confronted the staff throughout the locating effort discussed below.

Case No. 1: Mr. Charles

Information obtained from law enforcement and correctional agencies disclosed that Mr. Charles was being sought not only by the police but also by the department of parole and probation for offenses and charges falling within their respective jurisdictions. These difficulties had begun in August, 1971, when Mr. Charles was arrested for violation of narcotic laws in his city. At the time of his arrest, he was on parole after serving part of a ten-year sentence for a similar offense.

By obtaining bail shortly after his 1971 arrest, Mr. Charles managed to be released before his parole officer discovered that he had been in custody. As a result, a warrant for his arrest was issued charging violation of parole, as well as a warrant involving his failure to appear on the new charges of violation of narcotic laws. These warrants were still outstanding when we began our search for Mr. Charles.

Turning to local addicts for information, we learned that Mr. Charles was living in New York in Harlem, and was "dealing hard." (This meant not only that he was dealing hard drugs -- heroin and cocaine -- but also that he was hard at dealing.)

If Mr. Charles was indeed in New York, it seemed possible that Mr. Raymond, the study staff's special contact there, could help find him. Accordingly, two staff members and a locator, accompanied by a trained interviewer, drove to New York City in early December, 1973, and attempted to contact Mr. Raymond. Finding that he was

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* All cases reported here are actual experiences encountered by the staff of a recent survey in an American metropolitan area, although the subjects' actual names have not been used.

not at his home in Harlem, we left a note telling him that we would call back, and then went alone to an area that we knew was both a major marketplace for heroin and a meeting place where local and out-of-state addicts congregated. Within minutes we learned from addicts personally known to us that Mr. Charles was living in New York. We were told that only a few days earlier Mr. Charles had absconded with a large sum of money given to him by "some bad dudes" in a heroin transaction. No one had seen him since, and word was circulating through the neighborhood that he would be shot on sight if he had spent or otherwise misused the money entrusted to him. No one to whom we spoke would admit to any knowledge about Mr. Charles' hiding place, or about his source for the drugs he needed for his ongoing habit. A prostitute in the crowd commented that since Mr. Charles had a "dealer's habit," he could not stay away from the scene too long.

We did not tell his acquaintances that we wanted to interview Mr. Charles and felt that under the circumstances it was best to leave no messages at all. Before leaving, however, we obtained an address for another study respondent who reportedly lived in the area. The address proved correct, and the subject was located and interviewed that evening in the hotel where we were staying.

The next morning we were successful in contacting Mr. Raymond, who knew all the circumstances of Mr. Charles' disappearance. He told us that he did not know where Mr. Charles was hiding, but promised to try to find out. Together the locator and Mr. Raymond spent the next several hours hunting for information about Mr. Charles, but to no avail. Before parting from Mr. Raymond that night, we asked him to call if he learned anything new -- and around 2:00 a. m. Mr. Raymond telephoned.

Within the hour we and Mr. Raymond were inside a well known sleazy hotel in downtown Brooklyn which caters to prostitutes, pimps, addicts, and homosexuals. Mr. Raymond told the clerk that he wished to speak to the occupant of room 710 -- he was unsure whether Mr. Charles had registered under his own name, but he was certain that the room number was correct because "it came from a good source." He would not divulge this source, however.

The telephone was finally answered by Mr. Charles himself. He would not allow us or Mr. Raymond to go up to his room, but he was willing to meet us in the lobby, and did. Later he explained that he had company in his room and that his room was small.

We introduced ourselves, explained the study, and told Mr. Charles that the agency we represented would appreciate having his interview. We stressed the study's concern for confidentiality, and assured Mr. Charles that his whereabouts would never be divulged to anyone. We also informed Mr. Charles that we were aware of his legal difficulties in his home city, a gesture of honesty that seemed to reassure him. Apparently feeling that if we intended to do him harm we would have done so already, Mr. Charles agreed to cooperate and give the interview later that morning. At his request it took place in his hotel room at 10:00 a. m., and it went without incident except for the fact that his girlfriend refused to leave the room and permit the privacy that we would have preferred.

So far as is known at this time, Mr. Charles continues to be a fugitive from justice.

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ALCOHOLISM

Very often narcotic addicts become alcoholic as well. When this happens, they tend to move often from place to place, staying in one location only a short while before their alcoholism precipitates behavior that forces them to move on once more. Because of their unstable lifestyle and erratic behavior, they leave few traces behind; thus they are difficult to locate. The following account of Mr. William illustrates the study's experience in finding one highly mobile respondent who had developed serious alcoholism.

Case No. 2: Mr. William

Although Mr. William had been arrested in the 1950s and had served a short sentence, his jail record was so old it was inaccessible. Therefore we turned to police records, and learned that he had been arrested in another state after his local incarceration. A check of that state's police records revealed that the charge against Mr. William (molesting a minor) had not been pressed, and so he was released. Neither state nor local police had any information about him after his release. Two of his former employers and several individuals at the boarding house where he had lived were contacted, but none of them knew anything about Mr. William's current whereabouts.

However, one fact furnished by the police enabled us to locate Mr. William eventually: we learned through these contacts that he had become an alcoholic. Upon learning this, we identified several treatment facilities for alcoholics within a fifty-mile radius of Mr. William's last known location. The second one at which we inquired had indeed treated Mr. William and had records that included the names and addresses of some relatives -- but all were bogus. The personnel at the treatment facility reported that several months before, Mr. William had been released from their care and admitted to a half-way house elsewhere in the state -- from which he had been expelled (for drinking) on the same day he was admitted.

We then contacted the welfare department in the county in which the half-way house was located. Personnel there reported that Mr. William had applied for assistance from the county while he was hospitalized at a state hospital. From this hospital the study staff obtained an address for Mr. William, but checking it produced only the fact that Mr. William had moved on once more. Several additional addresses were found and checked out, but no useful information emerged until the trail led to a V.A. hospital. Mr. William had been a patient there, but left against medical advice -- apparently because he was accused of stealing money from another patient, something that had occurred more than once before in his past.

According to hospital records, Mr. William had presented himself at one time in the recent past to the alcoholic unit of the Salvation Army. From this source came information that Mr. William had previously been a patient in still another state hospital, so we checked there. We found that he had been readmitted and was at that moment still hospitalized there. A telephone call to his physician resulted in arrangements for an interview, which was obtained promptly by an interviewer who traveled from our office to the hospital and had a successful meeting with Mr. William.

RELUCTANCE TO COOPERATE

Some of the study subjects who are exceedingly hard to locate present an additional problem after they are finally found: they are reluctant to cooperate with the study. The fear, hostility, resistance, or simple indifference of some individuals can be very difficult to overcome -- and sometimes cannot be overcome. The following case shows how one interview arranged through unusual effort was nearly thwarted by intervening circumstances.

Case No. 3: Mr. Roberts

The only address available for Mr. Roberts was that of his father as given on Mr. Roberts' local police record. When we were unable to reach the father by telephone, we went to the home and learned from a neighbor that the senior Mr. Roberts had died a few months before. The neighbor reported that the son had come for the funeral, but she did not know where he lived. However, she did know that a lawyer was handling the estate, and she was able to give us the lawyer's name. Through the lawyer we learned that Mr. Roberts lived on the opposite coast.

By letter and then by phone, the study contacted Mr. Roberts and he agreed to be interviewed by one of our colleagues, a professor at a university near Mr. Roberts' home. At the appointed time, the interviewer went to Mr. Roberts' residence, found him there alone (and drinking), and began the interview. After about an hour of conversation, however, the interview had to be terminated. Mr. Roberts had become so drunk that he lost consciousness.

Several days later the interviewer returned to the house to make another personal contact with Mr. Roberts. This time he met Mrs. Roberts, a woman born and raised in the Orient, who was very suspicious of us and our motives. She felt that we were involved in a conspiracy to swindle Mr. Roberts out of his inheritance, and she urged her husband not to cooperate further with the interviewer or the study.

Mr. Roberts did not feel this way, however. He was actually very sympathetic with the study's purpose, and wished to make good his original promise to help. Without his wife's knowing it, we contacted him again and he made another private appointment with the interviewer, kept it, and completed the interview satisfactorily.

APPENDIX H

THE SELECTION AND SUPERVISION OF INTERVIEWERS

SELECTION OF INTERVIEWERS

It will facilitate the interviewing process if the interviewers are recruited from the area where the client population is to be found. The problem of recruiting interviewers is simplified by the fact that most clients receive treatment within a few hundred miles of the place they consider to be home. (Large V. A. or other federal government centers and hospitals may be an exception to this rule.) Experience also shows that most people, including people with drug problems, do not move far when they move, even if they move often. Recent studies have shown that 70 per cent of all clients treated in urban centers will still be living in the area two or three years later. Therefore, the survey administrator has little problem in deciding how to apportion interviewers geographically. If the center is near a metropolitan area to which a fair number of respondents may have moved, it would be wise to choose some interviewers who live in that area.

Recruiting Procedures

When programs recruit interviewers, ads in local newspapers and on radio stations will usually produce an ample supply of applicants. The following ad has been used successfully: "Part-time interviewing for __X__ Research Center. Men or women. Flexible schedule. Use of car necessary. No experience required. Call Ms. Jones, 243-6005."

In communities where it is wise to keep a low public profile, the ad can be blind: in such a case the ad tells the applicant to call a special number (assigned for this purpose), and to ask for a name (also assigned to identify callers as applicants). When applicants call in, the phone conversation should be kept as brief as possible: its main purpose is to get the applicant's name and address. Then materials should be sent to the applicants who called in. These materials should include the following: (1) a brief description of the job; (2) the pay rate; (3) the time requirements, by day or by week; (4) beginning and ending dates for the job; (5) any other conditions of work that are required, such as a car, availability during evening hours, or availability for travel; (6) any basic educational requirements, such as high school graduate or some college; and (7) an invitation to call for an appointment. Some callers will disqualify themselves upon receiving this material. Others will remain interested and call back for an appointment.

Recruitment Interviews

In the recruitment interview, applicants should complete application forms. They should also have the opportunity to ask questions about the job, and to get as much information as possible. At this time, applicants are also sometimes asked to complete a trial interview with a friend or a stranger, or both. You should have available a simple document, designed for this purpose, requiring the person to follow clear instructions. If the applicant does these tasks well, and has the other general characteristics needed for the study, this is probably a good person to hire. As a final step it is always a good idea to check the business references of a person prior to hiring.

It should be noted that in most instances, you will screen two or three times the number of people you plan to hire.

Paying Interviewers

Once you have actually hired a prospective interviewer, you should pay for all training and preparation for interviewing. In 1976, starting rates for interviewers were approximately $4.00 per hour.

In the past, some interviewing organizations have paid interviewers "by the case," a sort of piece work payment. This practice is becoming increasingly uncommon because of the potential legal difficulties it presents. Computations of overtime for this type of employee are difficult. For employees paid by the case, it is necessary to estimate the hourly rate and the number of hours represented by each case, so that an employee who has completed cases representing more than 40 hours per week can have overtime computed. Thus it is far simpler to pay interviewers for the hours they have worked. Experience shows that if interviewers are well supervised, they will spend their time wisely and work efficiently.

SUPERVISION OF INTERVIEWERS

Procedures for Validations

Validations should be carried out in a manner consistent with the promise of confidentiality that the interviewer made to the respondent during the interview. To prepare the respondent for a possible validation, the interviewer should tell him something like the following at the time of the interview:

> "My office may call you to check on this interview. This is a necessary part of the research. The person who calls will know that you have completed the interview, but will not know any of the answers you have given to me. You may be asked to answer a few questions again. This is a routine procedure -- your answers to the validator will be kept as confidential as the ones you have given me."

Some respondents within the group or sample may be uneasy about validation, particularly a respondent in fear of being found by the law. If these respondents are asked to sign some document, perhaps a receipt for payment, validation of the signature against the signature in treatment records may be sufficient. If validation by signature is not possible, the questions used to validate the interview with such respondents should be limited to the following: (1) verify the date of the interview; (2) determine how long the interview took (look for gross differences); and (3) ask a question that verifies the respondent's identity, such as his birthdate. Researchers should be aware that even the most unthreatening questions may cause anxiety in these respondents. They may wonder: "Why do they want to know what I was doing on THAT date?" Therefore, the validator must be prepared to reassure any respondent whose interview is being validated. In addition, it is important to recognize that the validation answers may not be the same as those in the interview, even when the interviewer is totally honest. For example, the interviewer may record that it took 90 minutes to complete the interview, but the respondent's memory may vary by 30 minutes in either direction.

For this reason, it is a good rule of thumb to limit the validation to questions to which the respondent certainly knows the answer and about which he has no reason to lie. Questions about his birth date, his mother's name, or whether he is married are better than questions about last year's income or the number of times he has been to the doctor in the last year.

Validations are usually completed by phone. Ideally, this should be done as soon as possible after the interview. If the respondent has no phone, it may be necessary to validate the interview by another personal visit. As a rule, validations should not last more than five to ten minutes. It is important that the validator should not know the original answers to the question, because this has been shown to bias the validation.

Even when the interview schedule disagrees with answers obtained in the validation, it should not automatically be assumed that the interviewer is dishonest. When discrepancies are found, the interviewer should be stopped from working for a day or two while several more interviews are validated, preferably those completed recently. If there is consistent evidence of discrepancy, then all the interviewer's work should be checked. However, if no such evidence is found, the interviewer should be allowed to resume work immediately. Some researchers increase the validation rate for interviewers who have had previous validation discrepancies as an extra safeguard.

APPENDIX I

ACADEMIC SURVEY GROUPS

Survey Research Laboratory
Arizona State University
Tempe, AZ 85281
602-965-5000

Research Triangle Institute
P.O. Box 12194
Research Triangle, NC 27709
919-549-8311

Social Science Research Institute
University of Maine
164 College Avenue
Orono, ME 04473

Survey Research Center
SUNY
Buffalo, NY 14214
716-831-1675

Survey Research Center
University of Michigan
P.O. Box 1248
Ann Arbor, MI 48106

Survey Research Program
University of Massachusetts
100 Arlington St.
Boston, MA 02116
617-542-7037

Survey Research Center
U. of California at Berkeley
2220 Piedmont Ave.
Berkeley, CA 94720
415-642-6569

Institute for Social Science Research
UCLA - 405 Hilgard
Los Angeles, CA 90024
213-825-0711

Bureau of Social Science Research, Inc.
1990 "M" St., N.W.
Washington, DC 20036
202-223-4300 ext. 204

Bureau of Social Science Research, Inc.
1990 "M" St., N.W.
Washington, DC 20036
202-223-4300 ext. 204

National Opinion Research Center
6030 South Ellis Avenue
Chicago, IL 60637
312-753-1300

Educational Assessment Center
University of Washington
Seattle, WA 98195
206-543-1170

Survey Research Laboratory
University of Illinois
414 David Kinley Hall
Urbana, IL 61801

Survey Research Center
York University
4700 Keel St.
Downsview, Ontario, Canada M3J 1P3
416-667-3022

Survey Research Laboratory
University of Wisconsin
610 Lowell Hall
Madison, WI 53706
608-262-3122

Institute for Survey Research
Temple University
1601 N. Broad St.
Philadelphia, PA 19122
215-787-8351

Cleveland Area Survey
Western Reserve University
Cleveland, OH 44106
216-368-2677

APPENDIX J

PROBING TECHNIQUES FOR INTERVIEWERS

PROBING WITH PRECODED QUESTIONS

The material in this appendix has been adapted from J. S. Morse, Interviewer Training Manual (Chicago: National Opinion Research Center, 1976).

The probing technique is used with both precoded questions, and open-ended questions, but in different ways. Interviewers should be fully familiar with these probing techniques. They may profitably be practiced during any training classes for interviewers.

Procoded questions are used in questionnaires when researchers have a good idea of the range of possible answers to a question, such as whether the respondent's house is rented or being bought. Precoded questions are also used when researchers want the respondent (R) to place himself or his opinions in one of several descriptive categories. With precoded questionnaires, probing is used to focus the R's attention on the question and/or to direct the R's attention to the existing code categories. In all cases when the R does not respond with a code cateogry, the response should be recorded verbatim before it is probed.

When the Respondent Answers "Between Categories"

Occasionally the respondent will answer "between categories." For example:

> Q: Would you say your health is excellent, very good, good, fair, or poor?
> A: It's OK.

When this situation arises, probing teaches the respondent to be a respondent: by repeating the question and the categories, the interviewer indicates to the R that his job is to choose one of them. In this situation the interviewer has two distinct jobs: (1) to repeat all categories for the R, in order to avoid narrowing down his choice; and (2) to preface the repetition with some remark which indicates that the R needs to make a choice. The interviewer should never just rattle off the question again. For example, the interviewer might start with "well then, would you say ____," or "which comes closest ____," or "In general ____."

In every case the interviewer must be sure that the prefacing remark indicates to the R what to do. Thus, if the R says "It depends," a remark such as "In general ____" is appropriate because it tells the R that his generalizations on the subject are desired. On the other hand, if the R answers in his own words instead of using an answer category, the interviewer should use a prefacing remark such as "which comes closest ____." This will indicate that the interviewer is interested only in the specific categories listed on the questionnaire, and that the R needs to make a choice among these.

When the Respondent Misunderstands a Question

Listening carefully is a very important part of interviewing. There are several clues to alert the interviewer to the possibility that the R may have misunderstood the question, such as not answering within a code category, or answering with an incorrect category (an answer which incorporates part of an existing category, but is not the same). If it appears that the R misunderstood the question, the interviewer should acknowledge his response ("I see" is neutral acknowledgement) and repeat the question for him.

When the Respondent Answers "I Don't Know"

If the R says "I don't know," it may mean one of several things, and the interviewer should be alert to the various possibilities. Clearly, "Don't Know" is the easiest answer to any question. The respondent may not understand that the interviewer really wants a thoughtful answer. The best way to communicate this expectation to the respondent is to use what is called a silent probe. That is, the interviewer should simply pause and look expectantly at the respondent, indicating that "Don't Know" is expected to be a preface to an answer.

"Don't Know" may also mean "I haven't really thought about it." If this appears to be the case, the interviewer should reassure the respondent by communicating a willingness to wait while he thinks about it. A remark such as "I wonder if you could take a minute to think about it (repeating the question)" is appropriate. Finally, of course, "Don't Know" can mean just that: "I haven't the faintest idea." In order to be sure this is the case, the interviewer should always probe a "Don't Know" at least once. If the R still answers "Don't Know" (DK), the interviewer should code or record this answer as such and go on to the next question. (Many questions have a DK code categroy which is not read to the respondent.) It is important to remember that the point of probing a "Don't Know" is not to force the R to give an answer, but rather to give him the chance to think about a response.

If the R still has not chosen an existing code category after these various types of probing, the interviewer should record his final response verbatim and go on to the next question.

PROBING WITH OPEN-ENDED QUESTIONS

Open-ended questions are used when researchers do not feel they know the full range of possible answers to a question. For this reason, it is particularly important for the R to have the chance to provide complete information, and to say everything he has to say. In order to meet this need, two kinds of probing are required when open-ended questions are used. First, answers should be probed for clarity. Next, the interviewer must probe for additional information.

Probing for Clarity

Two tasks are involved. First, the R's response must be evaluated. Does the interviewer know exactly what he means? Will someone else reading his response have the same understanding of his meaning? If the answer is not clear, the interviewer must then decide how to probe it in order to clarify its meaning. It is well to recall in this context that open-ended questions tend to be very general, such as asking the respondent what he thinks, or why he feels a particular way. As a result, respondents tend to answer such questions in a general way, and to use general adjectives to describe situations and opinions. Thus, probing for clarity may mean asking for a more specific response, or for an explanation of a term.

Some examples of the need for clarification follow. As indicated, a good technique involves repeating the word or phrase that needs clarification to the R as a question: The location? Your mother?

Q: Why did you choose this place to live?
A_1: I like the water (which water?)
A_2: I like the location (why?)
A_3: I like the size (of what?)
A_4: It's convenient (to what? for whom?)
A_5: Because of the neighborhood (what about the neighborhood?)
A_6: Because of my mother (what about her?)

As these examples suggest, the best probes for clarity are the ones which tell the R exactly what the interviewer wants him to do, or needs to know:

What do you mean ____________________?
Could you be more specific ____________?
Could you tell me a little more about that?

After probing, the interviewer should evaluate the response to the probe in order to determine if the answer is now clear. If not, further probing may be necessary. In order to avoid making the R feel harassed, some prefacing remark might be helpful, such as "I want to be sure that I understand what you mean here." When the R has clarified a response, this is a good time for some positive feedback. Two examples of this technique might include "I see" and a nod of the head, or "That's the kind of specific information I'm looking for."

If the R's answers contain several points that need clarification, the interviewer should deal with these one at a time. The interviewer may preface the probing with a remark like "I'd like to talk a little about each of these things separately," and then probe. In such a case, the respondent may end up clarifying all his remarks without further probing. If not, the interviewer must continue to probe each item until all the answers are clear.

Making a judgment about whether an answer is clear is often difficult. In general, it is better to probe than not to probe. However, it is important to be sure to probe the answer to the specific question, rather than pose a new question to the respondent. For example:

Q. Why did you choose this place to live?
A. Because it's convenient.
Q. Convenient?
A. Yes, this apartment is only a 10 minute drive to my office.
Q. Why do you want to live close to your office?

This final probe is inappropriate because the question itself is about the reasons for a choice, rather than the reasons for the reason. New interviewers sometimes assume either that too much is understood or that nothing is understood. The best way to make a judgment about the need for probing is to understand the intent of the question and to evaluate the response in light of that intent. It is important to be sure to probe the point of the question.

Probing for Completeness or Additional Information

Once a clear answer has been obtained, the interviewer should probe for additional responses to the question. The best way to do this is to repeat the substance of the question as part of a request for further information. For example:

What else do you like ______________________?
What other reason did you have ____________?

Another good tactic provides positive feedback and at the same time encourages the respondent to be thoughtful. This involves reading back the response obtained thus far, combined with a question like the examples above. For example: "Let's see, you said you chose this place to live because it's only a ten-minute drive to your office. What other reason did you have for choosing this place to live?"

Other types of probes can also be valuable. A simple "What else?" is occasionally a satisfactory probe, but this should not be used too often because it tends to become mechanical: the R may forget the original question or become sidetracked. It should be noted that "Is there anything else?" is a leading probe because it can too easily be answered by a NO. It may also make the R feel that the interviewer is more interested in moving on to the next question than in hearing anything else the R might have to say.

To recapitulate: each additional response should be probed for clarity as necessary. Only when a clear and unambiguous response has been obtained should the interviewer probe for additional responses. The interviewer should continue probing for additional responses until the R indicates that he has nothing else to say on the subject. The interviewer should be sure to record his final remark.

One point is especially important: the interviewers should be instructed never to probe until the respondent says "That's all," nor should "That's all" be used in examples or mock interviews. Experience shows that interviewers tend to become obsessed with recording this phrase at the end of questions and thus lose sight of the fact that the goal is not to record "That's all" at the end of every open-ended question, but rather to be sure to probe until the R really has nothing else to say.

APPENDIX K

SAMPLE AGENDA FOR INTERVIEWER TRAINING SESSION

Day #1	8:30 A.M.	Welcome, introductions, overview of the training program.
	9:00 A.M.	The study, its history, its purpose and goals: why we are doing the study.
	9:30 A.M.	The respondents and privacy. The plan for anonymity of respondents and confidentiality of data.
	10:00 A.M.	Coffee break.
	10:30 A.M.	The respondents: who they are, the kind of experiences they've had as drug users and at the treatment center.
	12:00 P.M.	Lunch.
	1:30 P.M.	Content of the questionnaire: question-by-question review.
	3:00 P.M.	Break.
	3:30 P.M.	Review of interviewing techniques.
Day #2	8:30 A.M.	Mock interviews (interviewers will be assigned to a group to do mock interviewing using each other as respondents).
	10:30 A.M.	Coffee break.
	11:00 A.M.	Group reports: discussion of mock interview experiences.
	12:00 P.M.	Lunch.
	1:30 P.M.	Drugs: what they are and what effects they have (A qualified person will speak on this topic.)
	2:00 P.M.	The interviewer's tasks before the interview. Contacting and locating the respondents. Answering respondent's questions. Arranging the place for interviews. Arranging the time for interviews. Dealing with difficult situations: respondents who refuse; respondents who break appointments; respondents who can't afford to come to be interviewed.
	3:30 P.M.	Coffee break.
	4:00 P.M.	The interviewer's tasks after the interview. Paying respondents (if they are to be paid). Answering respondents' questions. Returning the questionnaire.

Day #3	8:30 A.M.	Interviews (interviewers will be assigned to a group to interview paid respondents) (one interview, two interviewers observing).
	10:30 A.M.	Coffee break.
	11:00 A.M.	Discussion of experiences while interviewing.
	12:30 P.M.	Lunch.
	2:00 P.M.	Life patterns of heavy drug users (discussed by a qualified person).
	3:30 P.M.	Interviews with a paid respondent (an interviewer who observed at 8:30 will interview, with two other interviewers observing).
Day #4	8:30 A.M.	Survey administration: record keeping.
	10:30 A.M.	Coffee break.
	11:00 A.M.	The interviewer's role in preserving confidentiality of data.
	12:15 P.M.	Lunch.
	1:45 P.M.	Interviews with paid respondents (the interviewer who observed both sessions on Day #3 will be the interviewer, with two observers).
	4:45 P.M.	Editing completed questionnaires.

APPENDIX L

INSTRUCTIONS FOR INITIAL FIELD STAFF CONTACT WITH CLIENT

Introducing Yourself in Person

When you speak to the respondent personally, give him your name and the name of your employer, and explain why you want to talk to him. The conversation should go something like this:

> Hello, I'm __________ from the _______X________ Medical Care Study. We are currently doing a health study and you have been selected as a respondent. I would like to arrange a convenient time to interview you. When would that be?

In many ways, it is easier to introduce yourself in person than it is by phone. If you are at the address of the respondent, ask to see him personally. If you are at the home of a contact, ask to see the person whose name you have. If that person is not home, go ahead and make your inquiries with the person answering the door. Always have your I. D. card or letter of introduction handy in case someone doubts your identity.

When you are face to face with the respondent himself, immediately give your name and state your purpose. Then confirm his identity -- you wouldn't want to interview the wrong person. A good question might be: In what year were you born? (To confirm the year, check your face sheet -- the sheet you should have for each respondent to list any information which might help in locating the respondent.) Be ready to answer the respondent's questions, if he has any after reading the letter. Be sure to give him plenty of time to verbalize them so that later he will not think of things he wished he had asked -- things that might be serious enough in his mind to cause him to break an appointment or to refuse to complete an interview.

If the respondent is concerned about confidentiality, tell him something like this:

> Everything you tell me is absolutely confidential. Your name will not even go on the questionnaire. All your answers will be put with those of other people like you and will be released in statistical form only.

If the respondent wonders how you got his name:

> The study was given your name by the treatment center in order to do this study. I have a copy of a law here that explains why they could give us your name. (SHOW LAW ALLOWING RELEASE OF RESPONDENT'S NAMES) Of course, everything you say is confidential. Your name doesn't even go on the questionnaire. Your answers will be combined with other people's answers and will be released only in statistics.

Some respondents may want to be sure you are not from the government. Show your I. D. card. If he still is not convinced, let him call the study office or the treatment center.

Often respondents may want to know what they will get out of the study, and what good it will do them. The proper answer is that they are making a very important contribution towards the solution to a serious problem in our society today. A great deal of money has gone into treating drug addicts and alcoholics; some of it has been wisely spent, some of it has not. Since this person has experienced at least one kind of treatment, what happened to him is important in deciding what kind of treatment will be used for people like him in the future. If he didn't like the treatment center or the kind of treatment he received, explain that you need that information from him, but that you have to ask him the questions in order for his opinions to be included. Otherwise, all the people like him that he represents will be left out. Their experiences, as reflected in his answers, need to be included in the study so that it will reflect both good and bad experiences in treatment centers. Tell him that the results of the study will play an important role in the design of future treatment programs.

In summary, make your answers to the respondent's questions brief and to the point. Your job, in explaining the study to the respondent, is to observe the fine line between too much and too little explanation to the respondent. Too much explanation will make him nervous -- he won't understand why you are defending points that haven't occurred to him as needing defense. But too little explanation will leave him uncertain as to your identity, what you want, and why he should help you.

Introducting Yourself on the Phone

If you are calling the respondent's home, ask to speak to him personally. If he is not in, find out when he would be available, or where you could reach him. If the person who answers the phone wants to know who you are, give your name and say you are from the ______X______ Medical Care Study. If the party wants to know why you are calling, say that the respondent has been selected by a random process to participate in a health study, and you would like to get in touch with him to arrange for an interview. Give out only as much information as necessary so as not to cause worry on the part of the respondent or his family, but give enough to gain the cooperation of the person you have contacted.

If you are calling a contact, or reference, rather than the respondent himself, ask for the party with whom you wish to speak. When you reach that person, identify yourself by name and identify the study you are from. Ask if the contact knows anything that would help you locate the respondent, and tell why you want to locate him. Be as friendly and cordial as possible. Sometimes, contacts will worry that you are interested in obtaining information about them personally. Assure them that such is not the case. Sometimes a family member will not give out any information about the respondent until he or she is assured that you should have such information. At this point, tell them that the respondent has been selected to participate in a health study, that his name was picked randomly, and, most importantly, that any information given is confidential. If you can get nothing from the contact, leave your name and phone number so that the respondent can get in touch with you.

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